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CHEMISTRY,

AND

THE ARTS.

VOL. IV.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

ON the Completion of this fourth Volume I have the Pleasure to give the Names of the Authors and List of Engravings.

The Authors of original Papers are John Gough, Esq.; N. N.; J. Fletcher, Esq.; Ez. Walker, Esq.; J. Harriott, Esq.; Mr. W. Wilson; Dr. Priestley, F. R. S. &c.; Thomas Young, M. D. F. R. S. &c.; Mr. Benj. Hooke; W. N.; John Bostock, M. D.; G. C.; R. B.; The Rev. Wm. Gregor; Mr. Wm. Henry; Z.; π; and Lawson Huddleston, Esq.—Of foreign Works, G. Dalarive; Rochon; Guiton; Gadet; Gall; Bojames; Klaproth; Jumilhac; Saint Victor; J. B. Berard; L. Reynier; Proust; and Fourcroy.—And of English Memoirs abridged or extracted, R. Chenevix, Esq. F. R. S.; H. Davy, Esq.; S. Tennant, Esq. F. R. S.; Mr. J. Dalton; W. H. Wollaston, M. D. F. R. S.; W. Herschel, L. L. D. F. R. S.; R. Kirwan, L. L. D. F. R. S.; and M. des Lozieres.

Of the Engravings the Subjects are, 1. A new sliding Stop for Air-Pumps and other Uses. 2. A simple and secure Metallic Joint for Tubes. 3. Mr. Harriott's Syphon Engine for raising and lowering Weights. 4. New Method of measuring the Action of Bodies in refracting Light, whether they be transparent or opake; by Dr. Wollaston. 5. Mr. Hooke's Blow Pipe by Alcohol. 6. The Harmonic Sliders of Dr. Young. 7. A strong and flexible Joint for Tubes.

8. Dr.

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Soho Square, April 28, 1803.

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JANUARY, 1803

ARTICLE I.

On the Nature of the Grave Harmonics. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

OUR ſenſe of time ariſes, as Mr. Locke juſtly obſerves, from the conſtant ſucceſſion of ideas in the mind; or to ſpeak perhaps in more intelligible language, it is the reſult of the attention being occupied by an uninterrupted train of changeable perceptions. Time is on this account, capable of increaſe and diminution; it is therefore a ſpecies of the abſtract term, Magnitude; in conſequence of which, the parts of it poſſeſs all the properties contained in the abſtract term, Ratio, and are proper objects of the doctrine of proportion. A great variety of proportion takes place amongſt the conſtituent parts of compound beings of the ſame denomination, and gives birth to a claſs of phenomena in the philoſophy of the human underſtanding; which can only be explained by an hypotheſis aſcribing to man a faculty that compares theſe ratios, and perceives the effect produced by them. Of this deſcription are the differences perceived by the ſenſe of feeling in the texture of bodies,

Time is a ſpecies of magnitude; and its parts have the habitude of ratio to each other.

The mind compares ratios together as well as their parts. Phyſical effects thus diſcriminated.

ties, the diſcrimination of perſons, which is the office of the eye, and thoſe modifications of time, called ſymphony and harmony, which are judged of by the ear.

The ear affords the conception of a permanent ſound, only when the vibrations are too frequent to be ſeparately conſidered;

Each ſenſe reckons time, by the train of its own ideas alone; the auditory organ on this account meaſures the lapſe of it by ſound. Now if the ear had the power of conſidering the ſmalleſt intervals of time apart, it would perceive the conſtituent vibrations of ſounds as ſeparate diſtinct things, attracting its notice ſucceſſively; in which caſe, the human mind could not poſſibly form a conception of a permanent ſound: but it is a maxim in harmonics, that a certain number of vibrations in a ſecond is required to give a ſenſible degree of duration to the note of a ſtring; I ſhall at preſent make uſe of 12 for this number. The preceding fact aſcertains an eſſential point of my theory, for it fixes the leaſt interval of time that the ear can contemplate apart; this organ therefore takes in the groſs all magnitudes of this deſcription, which are too ſmall to be examined in detail: in this caſe it acquires the ideas of acute and grave, by comparing two or more trains of vibrations; the intervals of which are equal in each ſeries taken ſeparately, but greater and leſs in the whole number taken together.

but the ratios of frequency are perceived, and excite ideas of acute and grave.

Two continuous ſounds, heard together, have their vibrations arranged in cycles of equal duration perceptible by the ear: If the duration be long enough, each cycle is diſtinguiſhed;

When two ſounds, thus conſtituted, are heard in concert, the vibrations producing them are arranged in cycles, no one of which continues for a longer or ſhorter time than the reſt; and their effect is perceived by the ear, which becomes ſenſible of their preſence. For when each cycle of a ſeries, ſeparately conſidered, exceeds the twelfth part of a ſecond, the ſenſe of hearing recognizes each point of diviſion made by the coincidence of the vibrations which ſeparate the contiguous cycles: this circumſtance enables the ſenſe to contemplate theſe periods apart, and to comprehend their origin. On the contrary, when the duration of a cycle belonging to a compound ſeries does not exceed the twelfth part of a ſecond, the interval proves too ſmall to be meaſured by the ear; it therefore eſcapes notice in a ſeparate ſtate; for the points of diviſion recur too frequently to be obſerved. When the auditory organ finds itſelf in circumſtances anſwering to the preceding deſcription, it has but one method to purſue; which is to treat theſe derivative intervals in the ſame manner it treats all periods which are ſingly too ſmall for its comprehenſion: it therefore reduces them to a ſimple muſical ſound, correſponding

if not, the points of recurrence will have the effect of ſimple periods,

and afford a muſical ſound, or grave harmonic,

ing in pitch to a ſtring; which vibrates once in the time of each ſucceſſive cycle: A grave harmonic is on this account always a lower note than any of its conſtituents, ſeeing the time of a cycle exceeds the greateſt vibration that enters into the compoſition of it. The ſtrength of a grave harmonic is alſo weak, when compared with the notes compoſing it, becauſe theſe ſecondary ſounds, being nothing more than certain unavoidable efforts of the imagination, they aſſume the character of a feeble ſound, which is juſt ſtrong enough to be heard in the company of one or more louder tones; for the power of the imagination is always inferior to external impreſſions, except in fits of inſanity, when the organs of ſenſe appear to be blunted by phyſical cauſes. This inferiority of the fancy may be aſſigned as the reaſon of imaginary ſounds, which it frequently creates, being conſtantly faint and apparently diſtant.

Lower than either of the two original ſounds, and fainter. It is the reſult of imagination.

Dr. Young mentions a trait of the grave harmonics, which ſpares my own authority, and gives an opportunity to quote his for the following fact, which is of the firſt moment to the preſent theory: The grave harmonics always keep the direction of the ears, let the poſition of the head be changed as oft as you pleaſe, reſembling in this circumſtance a ſhrill piping note, called the ringing of the ears; which every one aſcribes to a ſlight affection of the auditory duct, becauſe it differs from external ſounds in having no fixed direction. The grave harmonics agree with the ringing of the ears in this remarkable particular; which is a ſtrong proof that their immediate cauſe is ſeated in the perſon of the hearer; and it is evident from the nature of things, that this cauſe originates in the mind, ſeeing that the organ does not labour at the time under a phyſical impediment.

In proof of which, the fact *that it has no direction* is urged.

I have now given a general theory of theſe ideal ſounds, in compliance with Dr. Young's injunctions, but am not able to perceive any connection between the ſubject and the queſtion in debate. This was the reaſon why I diſregarded in my laſt, one of the Doctor's remarks, which he has ſince called a challenge, perhaps in a petulant manner: a little more argument and a little leſs ill humour would certainly promote both his cauſe and reputation. The Doctor endeavours to perſuade me, that I miſunderſtand his idea of coaleſcence. My idea of the ſubject is ſimply this, that ſounds cannot coaleſce; conſequently that all compounds are nothing but a number of diſ-

Concluding remarks.

tinct cotemporary founds: if the Doctor will condefcend to fay, he contends for nothing more, the difpute is at an end; but fhould he reject this propofal, I advife him to attempt the refutation of my paper by fober arguments, and to tranfmit his thoughts to that Society which publifhed mine; where, without doubt, they will meet with a candid reception.

JOHN GOUGH.

Middlefhaw, near Kendal,
December 13, 1802.

II.

Bagatelles relating to the Pneumatic Apparatus. Sliding Stop-cock; fimple and fecure metallic Joint for Tubes; Improvement of Read's Apparatus; original Invention of Woulfe's Bottles and Tubes; and of a Method of clofing Veffels. By N. N.

Invention of fliding ftop-cocks.

§ 1. THE prefent bagatelle writer has been frequently at a lofs, in his pneumatic experiments, to meet with ftop-cocks both cheap and perfectly air-tight, or to have them eafily repaired when out of order. In a converfation he had on this fubject with Dr. Fifcher, *ci-devant* Aftronomer at Manheim, he underftood that Mr. Helfenfrieder, while Profeffor of Natural Philofophy at Ingoldftadt, to remedy fimilar defects in his air-pumps, ufed fteel parallelopipedons with a vertical and horizontal opening joined at right angles, and fliding between brafs-work. This furnifhed the writer with the idea of conftructing fliding ftop-cocks for a gazometer; he was conftructing fimilar inftruments. He lays no claim to the invention; but has found them anfwer extremely well.

Defcription of the cock.

Fig. 1. Pl. I. exhibits a fection of the fliding ftop-cock put together.

Fig. 2. One of the brafs parallelopipedons (*a a*, Fig. 1) againft which the pieces *b* and *c*, Fig. 1, are preffed and united by means of four fcrews.

Fig. 3. The flider by which, when brought home on one fide, the ftop-cock is opened, and, on the contrary, fhut when moved flufh on the oppofite extremity.

N. B. The bore of the whole is three-eighths of an inch diameter throughout, which is of the greateft advantage, when any

any gas is immediately transferred from the disengaging vessel to the gazometer; for if the apparatus be sufficiently air-tight as it ought to be, and the gas rapidly disengaged, an explosion may be occasioned for want of a sufficient passage for the gas.

Fig. 4. The piece *c*, Fig. 1. Its companion *b* ends in a male screw to be united to the gazometer.

Fig. 5. A perspective view of the stop-cock, the above enumerated parts being put together.

Fig. 6. A nut provided with a female screw soldered to the gazometer, by means of which the stop-cock may be applied or removed as usual.

Fig. 7. represents two brass cylinders. One end *f* is fitted by grinding to the orifice of the stop-cock; and to the other ends *g* and *h*, between *x*, is fastened a flexible tube for experiments with the blow-pipe.

Cheap and very useful flexible tube for the gases.

This flexible tube was made by twisting a brass-wire spirally round a long thin cylinder, covering this with oiled silk twice wrapped round, and fastened by means of thread between the grooves of the wire. It was then again varnished and covered in a spiral manner with sheep-gut slit longitudinally, and again secured with thread. Lastly, to protect the whole from external injury, it was covered with leather in the same manner as the tubes of the inhalers. These flexible tubes answer the same purpose as the very costly ones made of elastic gum, similar to the hollow bougies made for surgeons.

Fig. 8. A (broken) blowpipe, put in this place merely to shew how its end *k* is connected with the gazometer, by fitting into the end *(f)* of the brass end of the flexible tube.

Description of a joint for metallic tubes.

§ 2. In pneumatic experiments for transferring any gas, for instance oxigen, from the air-holder to another vessel, it is frequently necessary to have tubes which can be joined in certain directions and inclinations; and for this purpose a contrivance has been adopted, which is well known, and is represented Fig. 1. Plate II. where *a* and *b* are spherical segments, and *c* their junction by means of a screw. But as this apparatus, besides that its parts are with difficulty well ground together, is liable to become imperfect, more especially by strong pressure, or a blow received on the edges of the segments, or a grain of dusty sand interposed between the small flat surfaces, the writer of these trifles has contrived another,

A better construction.

delineated

delineated Fig. 2. A B C are the parts correfponding to thofe of the preceding. It ferves as a connecting piece, to which tubes at D and H may be joined in the requifite directions. Its conftruction and advantages are fufficiently obvious from the mere infpection of the drawing, fo that nothing is requifite to be added on this account. Several friends had made it after this model, and highly recommend its ufe; and fince the making of it the writer underftands that the ingenious Mr. Webfter, whofe merits were never properly eftimated by the great improver of culinary utenfils, had the fame thought, and executed it about two years ago; he, therefore, does not claim the merit of its firft invention, but only (perhaps) that of publifhing it.

The fliding ftop-cocks, § 1, as well as thefe connecting pieces, § 2, have been executed in a mafterly manner by Mr. Hooke, optician and mathematical inftrument maker, No. 159, Fleet-ftreet.

Various improvements in Read's funnel apparatus.

§ 3. In Mr. Read's very fimple and cheap pneumatic apparatus (fee Nicholfon's Journal, new feries, Vol. III. page 55) two improvements have fince been made. The firft, that the top of the exterior tube is not foldered to the funnel I (Plate IV. *cit.*) but has a top fcrewed on with a collar of leathers, and the upper part of the innermoft tube A F projects above it 1½ inch, and receives the brafs cylindrical end of the funnel, fitted to it by grinding. The fecond improvement has been made by Mr. Hooke, who inftead of foldering all the parts together, has joined the interior parts to the outer tube A A, in the middle about E. By thefe means, if any impurities that might impede the action of the inftrument, fhould happen to fettle at the bottom C or D, or at L, the whole may be eafily taken afunder and cleaned.

Woulfe's apparatus was invented by Glauber.

§ 4. The whole chemical world fpeaks, hears, and writes of Woulfe's pneumatic apparatus. The tribute of merit muft not be denied to Mr. Woulfe; but perhaps it may be curious to know, that the original invention of this contrivance, in the ftrict fenfe, belongs to good honeft father John Rudolph Glauber. If any gentleman doubt of this fact, let him confult Glauber's works, tranflated into Englifh by Chriftopher Packe, and printed, Lond. 1689, in folio, for the tranflator, by Thomas Milbourn. The very firft plate to Glauber's Treatife on Philofophical Furnaces fhews it, Fig. 3.

§ 5. An-

§ 5. Another bagatelle of this kind, relating to pneumatic instruments, is a pretended new method of closing bottles and other vessels air-tight *without luting or grinding*, which, a few years past, I saw recommended (I do not recollect in which of our Journals), and which consists in having a groove round the neck into which a cap fits, see Fig. 3; so that this groove *(a)* may be charged with water or mercury as circumstances require. This likewise belongs originally to Glauber, as may be seen in the plate just before quoted of the translation of his works. Another old invention.

There is *nothing new under the sun*, says Solomon, *Eccles.* I. v. 10. and more of this kind of *old news* shall be occasionally given by,

SIR,

Your humble servant,

N. N.

December 12, 1802.

III.

Analysis of Corundum, and of some of the Substances which accompany it; with Observations on the Affinities which the Earths have been supposed to have for each other in the humid Way. By RICHARD CHENEVIX, *Esq. F. R. S. and M. R. I. A. From the Philosophical Transactions for* 1802.

§ I.

SOME kinds of corundum, such as the adamantine spar of China, and the sapphire, have already been analyzed by Mr. Klaproth. This would have rendered any further experiments unnecessary, were it not, that I have had at my disposal many kinds of corundum he did not possess, and also some substances accompanying it, which were unknown before the preceding communication of the Count de Bournon. Kinds of corundum not examined by Klaproth.

As, from the result of my analyses, it appears that all the different kinds of corundum are nearly similar in their constituent parts, and differ only in their proportions, it would be tedious to mention every experiment I made upon each kind. I shall therefore confine myself to stating, once for all, such modes All the kinds are similarly constituted.

modes of analyſis as were employed with ſtones of a ſimilar nature; and then preſent a ſummary of the reſults: laſtly, I ſhall conclude with an enquiry into a much conteſted point, which lately threatened a revolution in docimaſtic chemiſtry.

Principal character: Extreme hardneſs.

A principal character of corundum in general, as may be found in the Count de Bournon's mineralogical deſcription, is extreme hardneſs; and thence the difficulty of reducing that ſubſtance into fine powder will be eaſily conceived. We are told by docimaſtic chemiſts, that the moſt advantageous method of pulverizing hard ſtones, is to make them red-hot; and, in that ſtate, to plunge them into cold water. But I found that this operation, when performed but once, was by no means ſufficient for corundum. I therefore repeated it, till the ſtone appeared to be fiſſured in every direction. After this, the ſpecimen to be pulverized was put into a ſteel mortar, about three-fourths of an inch in diameter, and three inches in depth, into which a ſteel peſtle was very cloſely adjuſted. A few blows upon the peſtle cauſed the ſtone to crumble; and the fragments were then eaſily reduced into an impalpable powder, in an agate mortar, with a peſtle of the ſame material. The abraſion from the mortar, uſual in the pulverization of hard ſtones, was much diminiſhed by the above precaution; rubies and ſapphires being, in a ſhort time, ground to a powder nearly as minute as the fineſt precipitate.

Pulverized after repeated ignition and quenching.

Efficacy of the proceſs.

The great difficulty of fuſing corundum with alkali, renders that proceſs objectionable.

M. Klaproth in his analyſis before-mentioned, had obſerved with how much difficulty the ſtones were acted upon by potaſh or ſoda. I found that the greateſt heat a ſilver crucible could ſupport, without melting, was not ſufficient to produce a ſatisfactory fuſion of one part of corundum, with ſix parts of either of thoſe alkalis; nor did an expoſure to that temperature during ſeveral hours, ſeem to render the treatment more effectual. Not more than half the quantity of the corundum was ever rendered ſoluble in any acid; and what remained was the powder of the ſtone, wholly unchanged. The repeated filtrations and evaporations with which this treatment muſt be attended, not only render it tedious, but alſo produce uncertainty in the reſults. Even when very finely powdered corundum was expoſed, with ſix times its weight of potaſh, in a platina crucible, to a heat of 140° of Wedgwood, for two hours together, it was not acted upon in ſuch a manner as to be

be fit for analyfis. From all thefe experiments I concluded, that fome more efficacious mode of rendering corundum foluble in acids was to be fought.

I boiled a great quantity of fulphuric acid upon very finely powdered corundum in a platina crucible. But, although the acid, after a great length of time, had diffolved a little of the ftone, I did not find this method more fatisfactory than the others. Nitric, muriatic, and nitro-muriatic acids, were lefs effectual than the fulphuric. Phofphoric acid, held in fufion with corundum, did not diffolve any notable portion of that ftone, or render it foluble in other acids.

Neither the volatile acids nor the phofphoric have any notable action on corundum.

I then had recourfe to fub-borate of foda (borax), which I found to anfwer beyond my expectation. Two parts of that falt, calcined, and one of corundum, enter into fufion, at a temperature which I judged to be about 80°. of Wedgwood *; and a glafs, more or lefs coloured, is formed. This glafs is foluble in muriatic acid; and, by this method, it is eafy to obtain a complete folution of corundum. My general method of operating was as follows:

Borax fufes it well. Two parts of calc. borax and one of cor. form a glafs foluble in muriatic acid.

I took one hundred grains of corundum; and, having feveral times made it red-hot, and plunged it into cold water, I put it into the fteel mortar, and treated it as already mentioned. I then poured fome very dilute muriatic acid upon it, to wafh off whatever iron might have adhered, in confequence of its mechanical action upon the mortar. After it was dried and weighed, I put it into the agate mortar, and ground it as fine as I could. The augmentation of weight was then noted; and was always taken into account in the general refult. I then put the whole into a platina crucible, with 200 grains of calcined fub-borate of foda, and expofed the mixture for an hour or two to a violent heat. When the crucible was cool, muriatic acid was boiled upon it and its contents; and, in about twelve hours, all the glafs difappeared. If I wifhed to obtain the filica directly, I evaporated the whole to drynefs; but, if otherwife, I precipitated by an alkaline carbonate, and wafhed the precipitate, in order to get rid of all the falts contained in the liquor. This latter mode I believe to be preferable. I then re-diffolved the precipitate in muriatic acid, and evaporated for filica. But, as corundum contains only a fmall portion of this earth, there was

Analyfis. 100 corundum were pulverized; fufed with 200 borax; diffolved in muriatic acid; evap. to drynefs for filica, or) precip. by alk. carbonate; abftract the falts by water; rediffolve the precip. in mur. acid; evap. for filica; filter the mur. folution; precip. by heat with excefs of potafh; oxide of iron is permanently depofited, but alumina taken up by the alk.; precip. the alumina by mur. of amm. Wafh, dry, ignite, and weigh the earths.

* I have no doubt that a lower temperature would be fufficient.

was little or no appearance of jelly. When the ſilica was thus precipitated by evaporation, I filtered the liquor, and boiled it with an exceſs of potaſh. By this operation, the alumina was precipitated, and then re-diſſolved by the exceſs of potaſh, from which it was finally obtained by muriate of ammonia; the iron which had remained undiſſolved by the potaſh, having of courſe been previouſly ſeparated from the alumina. This earth, and the ſilica, after being waſhed and dried, were ignited, and thus the weight of both was obtained.

Example. Analyſis of the ſapphire.

I ſhall exemplify, in a ſingle inſtance, this mode of treatment; and then preſent the reſults obtained from the different kinds of corundum. For this purpoſe, I ſhall ſelect the blue perfect corundum, or ſapphire, as the ſtone which has been the moſt ably analyzed by Mr. Klaproth. From a view of both analyſes, the efficacy of the fuſion with borax will be evident; and the reſults of the ſeveral experiments may be compared.

100 grs. ſapphire were pulverized and then fuſed into a green glaſs with 250 borax,

1. 100 grains of ſapphire, pulverized in the agate mortar, as above ſtated, had increaſed to 105. Theſe 105 were mixed with 250 of calcined ſub-borate of ſoda, and put into a platina crucible. They were then expoſed to a violent heat for two hours, and afterwards allowed to cool. The maſs was vitrified, and had the appearance of a greeniſh blue glaſs, fiſſured in many directions.

which was diſſolved in boiling mur. acid.

2. This glaſs being ſtrongly attached to the platina crucible, the whole was put into muriatic acid, and boiled for ſome hours. By theſe means, a total and limpid ſolution was obtained.

Precip. by carb. of ammonia, was waſhed, dried, diſſolved in mur. and expoſ. to evap.

3. The matter of the ſtone was next precipitated, by ammonia not entirely ſaturated with carbonic acid; the liquor was filtered; and the precipitate well waſhed and dried. It was then rediſſolved in muriatic acid, and evaporated.

it gave a precip. of 10.25 ſilica.

4. By this evaporation a precipitate was formed, which, when well waſhed and ignited, weighed 10,25 grains, and was ſilica.

The water of waſhing and the mur. liquor were boiled with exceſs of potaſh. One grain of iron remained.

5. The liquor, together with that which had waſhed the precipitate, was boiled in a ſilver veſſel, with an exceſs of potaſh; this rediſſolved all the precipitate, except one grain.

Mur. of amm. was added to the alk: ſolution.

6. Muriate of ammonia was poured into the alkaline ſolution. (No. 5.) The potaſh expelled the ammonia from the muriatic acid, and, forming muriate of potaſh, could no longer retain the earth in ſolution; a very copious precipitate, therefore, was formed.

formed. This precipitate had all the properties of alumina; and, when well waſhed and ignited, weighed 92 grains. Conſequently, deducting 5 from the ſilica, for the abraſion of the mortar, we ſhall have for reſult,

The potaſh left united with the mur. acid, and ſuffered the alumina to fall. Waſhed and ignited it weighed 92 grs.

Comp. parts deduced.

Silica - - - -	5,25
Alumina - - - - -	92
Iron - - - -	1
Loſs - - - -	1,75
	100,00.

The chief difference between theſe proportions and thoſe eſtabliſhed by Mr. Klaproth, is in the ſilica. That chemiſt did not find any notable portion of it in the ſpecimens he examined. This naturally induced me to make a very ſtrict reſearch into every poſſible means by which any ſilica might have been introduced into the reſults; whether by the borax, the alkali, or any of the other re-agents I had uſed. But, finding very clearly, that none of theſe ſubſtances did contain any, I could no longer heſitate to believe, that the proportion I have here ſtated, was actually contained in the ſapphire I analyzed. I am likewiſe convinced, that no more than the quantity I have mentioned was worn from the agate mortar and peſtle; for my conſtant practice was, to weigh them, both before and after I had uſed them, in ſcales which, when charged with four pounds on each end, turn eaſily with the tenth part of a grain.

As Klaproth found little ſilica in his analyſis; this point was particularly examined.

The general reſults, from all the different kinds of corundum, were as follows:

Component parts of diff. kinds of corundum.

Blue perfect Corundum, or Sapphire.

Silica - - -	5,25
Alumina - - -	92
Iron - - -	1
Loſs - - -	1,75
	100,00.

Red perfect Corundum, or Ruby.

Silica - - -	7
Alumina - -	90
Iron - - -	1,2
Loſs - - -	1,8
	100,0.

Imperfect Corundum from the Carnatic.

Silica - - -	5
Alumina - - -	91
Iron - - -	1,5
Loſs - - -	2,5
	100,0.

Imperfect Corundum from Malabar.

Silica - - -	7
Alumina - -	86,5
Iron - - -	4
Loſs - - -	2,5
	100,0.

Imperfect

Imperfect Corundum from China.		*Imperfect Corundum from Ava.*	
Silica	5,25	Silica	6,5
Alumina	86,50	Alumina	87,0
Iron	6,50	Iron	4,5
Lofs	1,75	Lofs	2,0
	100,00.		100,0.

Their diff. of colour arifes from the oxidizement of the iron.

As I could not difcover chrome, or any other colouring fubftance, except iron, in thefe ftones, I can attribute their difference of colour only to the different ftate of oxidizement of the iron; but it is impoffible to afcertain what that ftate may be, from fo fmall a quantity.

The matrices of thefe ftones fubmit to the ufual analyfis.

The matrices of thefe ftones, and the fubftances accompanying them, are more eafily fufed than the fix kinds of corundum juft mentioned. The ufual and well known mode of treatment by potafh, was fufficient to render thefe fubftances foluble in the acids. Since the many experiments of Klaproth, Vauquelin, and others, the mode of analyzing mineral bodies is become fo familiar to chemifts, that I fhall mention particulars with refpect to one only of the following fubftances.

MATRIX OF CORUNDUM FROM THE PENINSULA OF INDIA.

Matrix of Indian corundum. 100 grs. fufed with potafh; diffolved in mur. acid; evaporated; diffol. in fm. excefs of acid; left 42,5 of *filica*.

1. A certain quantity of this matrix was reduced to powder, in the manner already defcribed. 100 grains of it were treated with potafh, in a filver crucible: they then afforded a limpid folution in muriatic acid. The liquor was evaporated; and, long before the mafs was entirely dry, it had affumed the appearance of a jelly. When the faline matter in the evaporating-difh was diffolved in a flight excefs of acid, a white powder remained at bottom, which had all the properties of filica, and, when wafhed and ignited, weighed 42,5 grains.

Ammonia threw down alumina and oxide.

2. Into the liquor which had ferved to wafh the above powder, I poured ammonia. A copious precipitate was thus formed, which was feparated by filtration, and well wafhed.

Carb. potafh threw down carb. lime = *lime* 15.

3. Carbonate of potafh alfo caufed a precipitate in the liquor of No. 2. This precipitate was found to be carbonate of lime, and weighed 23,5 grains, = 15 of lime.

The alumina and oxide were diff. in mur. acid; and the

4. The precipitate of No. 2. was rediffolved in muriatic acid; then boiled with an excefs of potafh, and filtered. There remained undiffolved, 3 grains, which were iron.

5. The

5. The liquor of No. 4. was precipitated by muriate of ammonia, and afforded alumina, which, being washed and ignited, weighed 37,5 grains.

I could also perceive a trace of manganese.

The proportions therefore are,

Silica - - -	42,5
Alumina - - -	37,5
Lime - - -	15,0
Iron - - - -	3,0
Loss, with a trace of manganese -	2,0
	100,0.

Alumina taken up by potash. 3 grs. *iron* remained. Mur. amm. precipitated 37,5 *alumina*. *Manganese*, a trace. Comp. parts.

By a similar treatment, the following substances, contained in this matrix, afforded the under-mentioned results.

Felspar.

Silica - - -	64
Alumina - - -	24
Lime - - -	6,25
Iron - - - -	2,00
Loss - - -	3,75
	100,00.

Component parts of felspar;

Fibrolite.

Silica - - -	38
Alumina - - -	58,25
A trace of iron, and loss -	3,75
	100,00.

of fibrolite;

This is the only stone I have ever met with, that yielded nothing but silica and alumina; for the quantity of iron was so small as hardly to be taken into account. I have repeated this analysis three times, and have not found a difference of half a grain.

Thallite in Crystals, with a rough Surface.

Silica - - -	45
Alumina - - -	28
Lime - - -	15
Iron - - - -	11
Loss - - -	1
	100.

of rough thallite;

Thallite

of prismatic thallite;

Thallite in Prisms like the Tourmalin.

Silica - - -	40
Alumina - - -	25
Lime - - -	21,5
Iron - - - -	11,5
Loss - - -	2
	100,0.

of thallite in transparent fragments;

Thallite in Fragments, of a fine transparent Yellow Colour.

Silica - - -	42
Alumina - - -	25,5
Lime - - -	16
Iron - - - -	14
Loss - - -	2,5
	100,0.

of fibrolite of the Chinese corundum;

Fibrolite accompanying the Matrix of Corundum from China.

Silica - - -	38
Alumina - - - -	46
Iron - - -	13
Loss - - -	3
	100.

of felspar from sand of Ceylon.

Felspar from the Sand of Ceylon.

Silica - - -	68,5
Alumina - - -	20,5
Lime - - -	7
Iron - - - -	1,5
Loss - - -	2,5
	100,0.

Metallic crucibles alone must be used with earths;

As the greater part of the above substances were fusible without difficulty in potash, I preferred using a silver crucible to any other. It may be laid down as a general rule, with respect to delicate experiments, that in the treatment of metallic substances, we should not use metallic crucibles; but, in the treatment of earthy bodies, they alone are to be depended upon. The easily oxidizable metals cannot be employed; but silver and platina present advantages which no other metals seem

namely, silver and platina.

seem to possess. Theory would certainly give a general preference to platina, from its resistance both to heat and to acids; and practice will justify this preference, in all but a single instance. If a quantity of potash be kept for some time in fusion, in a platina crucible, it will be found that the crucible has lost several grains of its weight. The platina so dissolved may be looked for in the potash; and, if this be saturated with muriatic acid, and evaporated, we shall find the well-known triple salt, formed by the combination of muriatic acid with potash and oxide of platina. This action of potash upon platina, does not depend upon any mechanical cause, such as friction, the force that determines it being purely chemical. If a salt formed by potash, or a salt formed by ammonia, be mixed with a salt of platina, a precipitate ensues, which is a triple salt; and it is by this method, that the Spanish government detects the platina, in the ingots of gold sent from their American possessions. It is therefore evident, that an affinity does exist between potash and platina, in a certain state; and I imagine it to be this affinity, which causes the oxidizement of the platina, when potash is kept in fusion upon that metal. I must however observe, that my crucible was prepared by Janetty, in Paris, according to a method he has published in the *Annales de Chimie*: and that he always employs arsenic, a little of which certainly remains united to the platina. What influence arsenic may have, remains to be determined. Soda does not form a triple salt with the oxide of platina; for I have frequently kept this alkali in fusion, in a platina crucible, for a long time; yet very little action was produced upon the metal. This fact seems to corroborate my assertion, that the affinity of potash for oxide of platina, determines the oxidizement of the metal.

Platina is preferable; except with potash *which acts upon it.*

Spanish method of detecting platina in ingots of gold. The salts of platina are precipitated (triple) by the salts of potash.

Is platina affected by the arsenic it contains?

Soda scarcely acts upon platina.

Whenever I suspected that platina had been dissolved, I could easily detect the smallest portion of it. A solution of platina, so dilute as to be nearly colourless, manifests, in a very short time, the colour of a much more concentrate solution, and becomes reddish, by the addition of a solution of tin in muriatic acid. This I have found to be, by many degrees, the most sensible test for platina; and it would answer the purposes of the Spanish government, much better than that they usually employ.

Muriate of tin is the most delicate test of platina.

4 The

Alkalis do not act on silver; yet the crucibles become rather more brittle by long use.

The alkalis have no immediate action upon silver; but I have observed, that crucibles of this metal, after they have been a long time in use, become somewhat more brittle than they were before.

The fixed alkalis rise by mere heat; potash more readily than soda.

Potash and soda have long been termed fixed alkalis; and it is certain that, if we compare them with ammonia, they are so. But *fixed* is an absolute term, and cannot admit of degrees. If potash, such as we obtain from Mr. Berthollet's method of preparing it, be kept in fusion at a very strong heat, it may be totally volatilized. The vapour of the alkali may be perceived in the room; and vegetable colours will undergo the change which is usually produced by alkalis. Indeed, in preparing Mr. Berthollet's potash, the vapour of the alkali may be easily perceived. Soda is not quite so volatile; though far from being fixed. It appears also, that a little water increases the volatility of both potash and soda, as happens with boracic acid. This volatility of potash, has been advantageously applied of late to the art of bleaching.

Water assists their elevation.

Potash usefully volatilized in bleaching.

§ II.

On the Affinities the Earths have been supposed to have for each other, in the humid way.

In the course of the foregoing analysis, I had occasion to make some further observations concerning a subject upon which I had been formerly engaged, namely, on the affinities the earths have been supposed to have for each other, when held in solution by acid or alkaline menstrua.

On the affinities of the earths for each other.

Historical facts.

In the XXVIIIth volume of the *Annales de Chimie*, page 189, I published a paper upon the analysis of some magnesian stones. In this paper, I took notice of the following affinities of the earths for each other, namely, the affinity of alumina for magnesia, of alumina for lime, and of alumina for silica. In the XXXIst volume, page 246, there is a memoir, by Guyton de Morveau, upon a similar subject;* and he there reports some experiments of his own, by which he was induced to think, that the earths do really possess a chemical attraction for one another. Since that time, the affinity of the earths has been

Guyton's experiments.

* He has taken no notice of any of the experiments contained in my paper.

received

received among chemists as an undoubted fact; and, at the end of Mr. Kirwan's *Essay on the Analysis of mineral Waters*, we find a list of earthy salts which produce a re-action upon one another, supposed to be caused by an affinity that tends to unite their bases, in the form of a precipitate, insoluble in the acids. Some other detached observations are to be found, in the *Journal de Physique*, and in the *Annales de Chimie*. The fact is certainly one of the most important in the docimastic art, and merits all the attention of the skilful in that branch.

In the XLth volume of the *Annales de Chimie*, page 52, Darracq has published a paper, intended as a refutation of the conclusions drawn by Guyton. I had myself repeated the greater part of the experiments of the latter; and the results I obtained were exactly similar to those of Darracq. In fact, I had intended to continue the researches; but the very satisfactory paper of Darracq, appeared to me to render a further prosecution of them totally useless. However, a paragraph inserted in the *Annales de Chimie*, (Tom. XLI. p. 206.) and of which Guyton appears to be the author, shews that he has not derived from the Memoir of Darracq, that conviction which it certainly conveys. The paragraph in question is founded upon a letter, written from Freyberg, by Dr. G. M. to Dr. Babington, dated December 17, 1800, and inserted in the IVth volume of Nicholson's Journal, page 511. This letter contains an opinion which deserves to be canvassed, as it is not perfectly just; and the use Guyton has made of it, has determined me to add my observations to those of Darracq.

contested by Darracq.

I shall follow the order of Guyton's experiments, in the enumeration of those I made.

Repetition of Guyton's experiments.

Exp. 1. From a mixture of lime-water and barytes-water, Guyton obtained a precipitate. I obtained none.

Lime-water and barytes-water = precipitate.—Failed.

Exp. 2. A solution of alumina in potash, mixed with a solution of silica in the same, gave a precipitate, after standing some time. This had been observed by Darracq, and by Guyton, and agrees perfectly with the affinity which, before Guyton published his paper, I had asserted to exist between these two earths.

Alumina in potash and silica in potash = precip.—Ascertained before.

Exp. 3, 4, 5. Lime-water, strontia-water, and barytes-water, produce a somewhat similar effect upon a solution of silica in potash.

Water of lime, or of strontia, or of barytes + to silica in potash = precip.

Barytes water, and ftrontia-water = no precipitate.

Exp. 6. No precipitate took place from a mixture of barytes-water and ftrontia-water; nor from folutions of the carbonates of thofe earths, in water impregnated with carbonic acid.

Mur. lime and mur. alumina = precip.—Failed.

Exp. 7. Guyton obtained a precipitate, by mixing folutions of muriate of lime and muriate of alumina. I could not obtain any.

Mur. lime and mur. magnefia, = no precip.

Exp. 8. Solutions of muriate of lime and muriate of magnefia, when mixed, did not afford a precipitate.

Mur. lime and mur. barytes = precip.—Failed.

Exp. 9. Muriate of barytes did not, as Guyton has afferted, form a precipitate with muriate of lime. He was right in faying, that muriate of ftrontia gave no precipitate with muriate of lime.

Mur. mag. and mur. alum, = no precip.

Exp. 10. Muriate of magnefia and of alumina, afforded me no precipitate. Guyton fays, that the liquors became milky.

Mur. magnefia and mur. of barytes or ftrontia = abundant precip.—Failed.

Exp. 11. Muriate of magnefia, whether mixed with muriate of barytes or of ftrontia, afforded me no change; although Guyton fays he obtained an abundant precipitate, by mixing muriate of magnefia with muriate of barytes.

Mur. alum. and mur. barytes = precip.—Failed.

Exp. 12. Muriate of alumina and of barytes, did not, when mixed together, yield any precipitate. Guyton afferts, that there is a precipitate in this cafe.

Mur. barytes and ftrontia = no precip.

Exp. 13. Muriate of barytes and of ftrontia, did not form a precipitate. Guyton has remarked the fame.

Mur. ftrontia and alumina = no precip.

Exp. 14. From muriate of ftrontia and of alumina, I obtained no precipitate. With Guyton the liquor became milky.

Guyton was wrong in afferting that barytes has affinity for lime, magnefia and alumina; and that ftrontia attracts alumina.

From all thefe experiments it appears very clearly, that Guyton has pronounced too haftily, upon the affinity which he fuppofes barytes to entertain for lime, for magnefia, and for alumina; and that he is equally in the wrong, with regard to the affinity of ftrontia and alumina. With regard to *Exp.* 3, 4, and 5, although they appear to be true, yet it would require the refpective precipitates to be further examined, before we admit a decided affinity between the earths. The quantity of carbonic acid alfo, which muft of courfe combine with the potafh, during the treatment of the filica by that alkali, fhould be taken into account, in confidering the caufe of the precipitate.

The

The ſolutions which I uſed, of all the above ſalts, were in the moſt concentrate ſtate; therefore, in the ſtate moſt favourable for ſhowing precipitation, if any had taken place.

It is not very difficult to account for the appearances that deceived Mr. Guyton in his experiments, and for the cauſe that produced them. In one inſtance, he obtained a precipitate from muriate of lime and of alumina, becauſe, in all probability, the alumina he diſſolved in muriatic acid had been precipitated from alum; and alumina, thus prepared, retains a ſmall portion of ſulphuric acid *. In the next place, it is very likely that his ſolutions were ſufficiently concentrate to give a precipitate of ſulphate of lime. The ſame was the caſe with regard to his mixture of muriate of ſtrontia with muriate of alumina. As to the general concluſion, that barytes has an affinity for lime, magneſia, and alumina, which ſtrontia does not appear to poſſeſs, it is to be explained as follows. Lime often contains a little ſulphate of lime. Mr. Guyton's magneſia, as well as his alumina, had probably been obtained from the ſulphate; and we are indebted to Mr. Berthollet, for the true nature of many ſimilar precipitates. Cauſes of the errors in his experiments

Barytes is a much more delicate teſt than ſtrontia, for ſulphuric acid; and therefore, barytic ſolutions were affected by quantities of ſulphuric acid, which ſtrontia could not render ſenſible. This I have aſcertained to be the caſe: for I have obtained copious precipitates, by barytes, in a liquor compoſed for the purpoſe, wherein ſtrontia did not produce the ſmalleſt cloud, or ſhow the preſence of ſulphuric acid.

A little care and attention are neceſſary, in preparing the earths, which are to be diſſolved in the muriatic acid, for theſe experiments; and, if Mr. Guyton had taken the requiſite precautions, he would not have been led into error. The object to be kept in view is, to free the earth from ſulphuric acid; and, if this be obtained, there is not the ſmalleſt precipitate or cloud, in any of the caſes I have mentioned. If He did not take due care in preparing his earths.

* It is ſomewhat ſingular, that Guyton ſhould have obſerved this fact elſewhere. See his experiments on the diamond, in the *Annales de Chimie*. The preparation of a barytic ſalt, by alumina prepared from the ſulphate of this earth, had been obſerved by Scheele, in his *Eſſay on the Affinities of Bodies*. But that great chemiſt referred the phenomenon to its right cauſe, viz. to ſome ſulphuric acid remaining in all alumina thus prepared.

any further proof be neceſſary, with regard to the cauſe of precipitates obtained in the manner ſtated by Mr. Guyton, I may add, that I have repeated his experiments, and have always found the precipitates to be ſulphate of barytes.

Mr. Kirwan has made ſimilar miſtakes.

The general concluſion to be drawn from the obſervations of Mr. Kirwan, already alluded to, is, that barytes has an affinity for lime, magneſia, and alumina, upon which earths ſtrontia does not ſeem to have any influence. But theſe miſtakes are to be accounted for in the ſame manner as thoſe of Mr. Guyton, viz. by ſulphate of barytes being much leſs ſoluble than ſulphate of ſtrontia, and therefore ſhowing the preſence of a ſmaller portion of ſulphuric acid, or, in other words, being a much more delicate teſt for that ſubſtance.

Letter in Nicholſon's Journal,

With regard to the letter already mentioned as being inſerted in Nicholſon's Journal, and which drew ſome reflections from Mr. Guyton, it is neceſſary to examine as much of it as may be thought objectionable.

aſſerting that becauſe a precipitate of ſilica is ſoluble in acids, the reſults of analyſis with alkalis muſt be fallacious.

The author ſays,* that he repeated the experiments of Mr. Guyton, with an alkaline ſolution of ſilica and alumina, and that he obtained a precipitate; which precipitate, though containing ſilica, was totally ſoluble in the acids. "Here," he ſays, "the properties of the ſilex muſt be conſiderably altered. This muſt render all analyſis with alkalis ſuſpicious; and ſhews on what fallacious grounds the proud dominion of chemiſtry reſts, which ſhe has exerciſed ſo long, in ſuch an arbitrary and overbearing manner, in the mineral kingdom." This opinion is by no means likely to overthrow the pretenſions of chemiſtry; for the very circumſtance of rendering ſilica ſoluble in the acids, is one of the diſcoveries that has moſt contributed to render certain, and to extend, our knowledge of analyſis. No earthy ſubſtance is now thought fit to be ſubmitted to further experiment, till a complete ſolution of it in an acid be firſt obtained; and, when that ſolution cannot be effected directly by the acid, it is always attempted by previous fuſion with an alkali. This mode of rendering ſilica ſoluble in acids, is no new diſcovery; it has been long known; and the analyſis of minerals has never been brought ſo near to truth, as ſince it has become an indiſpenſable condition.

This fact is on the contrary the greateſt improvement in modern analyſis;

Alumina attracts ſilica;

I have no doubt as to the fact of a precipitate being formed, by mixing together an alkaline ſolution of ſilica and alumina. Alumina indeed appears to exerciſe an attraction, as I before ſtated,

ftated, for filica, for magnefia, and for lime. All ftones in which there is but little alumina, and a great quantity of filica, leave, after fufion with potafh, a light and flocculent fubftance, which cannot be diffolved by the acids: this fubftance, however, which is filica, has been in folution in the alkali. But, if a greater proportion of alumina be prefent, none of this flocculent precipitate appears; hence it is evident, that alumina muft determine its folution. Its eafy folubility, in the latter cafe, cannot depend upon the divifion of the particles of the filica in the ftone; for, in the firft place, after being fufed with potafh, the tenuity of the particles of every ftone muft be nearly the fame; and, in the next place, I have not obferved, that any earth, except alumina, can promote the chemical folution of the filica, though they muft all occafion its mechanical divifion.

And promotes its folubility.

As to the affinity of alumina for magnefia, it is by much the moft powerful of all thofe which any of the earths have for each other. I attempted to precipitate magnefia from muriatic acid, by ammonia, even in excefs; but found that the whole muriate of magnefia had not been decompofed, and that a triple falt, or an ammoniacal muriate of magnefia *, had been formed. I then poured an excefs of ammonia into a folution of muriate of magnefia, mixed with a large proportion of a folution of muriate of alumina. All the earth was precipitated; and nothing remained in folution, except muriate of ammonia. The liquor was then filtered, and the precipitate wafhed and dried. I diffolved it in muriatic acid, and boiled it with a great excefs of potafh. Some alumina was taken up, but by no means all the quantity that had been ufed. The precipitate which had refifted the action of potafh, was again diffolved in muriatic acid, and precipitated by carbonate of potafh. The carbonate of magnefia was held in folution by the excefs of carbonic acid; and, by ufing potafh and carbonic acid alternately, (the firft to diffolve alumina, the fecond to diffolve carbonate of magnefia,) I effected a feparation of the earths. Thefe experiments fhew, that there is an affinity between alumina and magnefia, and a certain point of faturation, where the action of potafh upon alumina is wholly counteracted by the affinity of that earth for magnefia.

Alumina very powerfully attracts magnefia.

* This falt is well known in chemiftry.

When

Alumina attracts lime; which becomes dissolved in potash along with it.

When a solution of potash is boiled upon a mixture of lime and alumina, the alumina is dissolved, together with a much greater portion of lime than can be attributed to the dissolving power of the water alone. But, if a solution of potash be boiled upon lime, without alumina, no more lime is taken up than would have been dissolved by an equal quantity of water not containing potash in solution; consequently, alumina seems really to promote the solution of lime in potash. The affinity of alumina for lime, I had mentioned in the paper to which I allude; and it has since been noticed by Mr. Vauquelin *.

Guyton's conclusions, if true, would have rendered analysis uncertain.

If the conclusions of Mr. Guyton had been well founded, it would have been chemically impossible to arrive at truth in analysis. There were already real difficulties enough to be overcome; and Mr. Berthollet has lately discovered some, which are not so easily answered as those I have just considered. The position of this chemist, however, has been too generally extended by him. If the power of masses were as great as he represents it to be, and if it increased *ad infinitum*, in proportion to the mass, it must follow, that, with any given substance, we could decompose any compound, provided the mass of the decompounding body were sufficiently great; but this is well known not to be the case.

Objection to Berthollet's unlimited position respecting attraction from the mass.

From the experiments which I have related, it appears to be proved,

Recapitulation of the affinities of the earths, &c.

1st. That there exists an affinity between silica and alumina.

2dly. That there exists a very powerful affinity between alumina and magnesia.

3dly. That alumina shews an affinity for lime; but that the said affinity is not so strong as Mr. Guyton had supposed, nor, if pure reagents be used, is it to be perceived under the circumstances stated by him.

4thly. That Mr. Guyton was mistaken in every instance of affinity between the earths, excepting in the case of silica with alumina, which had been observed before his experiments; and that, in the other cases, he has attributed to a cause which does not exist, phenomena that must have resulted from the impurity of his reagents

* Scheele was, in fact, the first who perceived this affinity. See his *Essay on Silex, Clay, and Alumina.*

5thly,

5thly. That neither the experiments of Mr. Guyton, nor the opinion maintained in the letter from Freyberg, are sufficient to diminish, in any degree, the value of the affiftance mineralogy derives from chemical inveftigation.

IV.

A Memoir on the Mufical Sounds produced in Tubes by Hidrogen Gas. Read to the Society of Philofophy and Natural Hiftory at Geneva. By G. DELARIVE *.

IN a former meeting our learned colleague, Profeffor Pictet, communicated to the Society a feries of experiments on the *mufical founds produced in tubes by hidrogen gas,* in which he developed the various mufical phenomena which thefe tubes produce. He explained the effect which the length or width of the tube, and the fituation where the hidrogen is burned, have on the founds produced. As to the caufe of the found, he gave only a few conjectures; his inquiries not being directed to that object. The purpofe of the prefent memoir is to difcover this caufe.

Profeffor Pictet's refearches on mufical tubes by hidrogen.

Profeffor Brugnatelli is, I think, the firft who publifhed the experiment I have endeavoured to explain †: It was invented by a German. I fhall here relate the principal circumftances.

Publication by Brugnatelli.

If a current of inflamed hidrogen gas be inclofed in a tube of an elaftic and fonorous material, fuch as glafs, metal, dry wood, &c. the tube after an interval of fome feconds, will produce a mufical found. If it is open at both ends, the found will be ftrong and full. It is poffible however to fucceed with a tube hermetically clofed at one end, provided its diameter be large enough to allow of a circulation of atmofpheric air fufficient to keep the gas in a ftate of combuftion. The conditions effential to the fuccefs of the experiment are, *firft,* That the tube be elaftic and capable of forming an echo, that is to fay, capable of reflecting the undulations which proceed from the fonorous point; for no found will be produced with a tube of pafteboard or paper: and, *fecondly,* The flame muft be produced by

Defcription of the manner of burning hidrogen in a tube, to produce a mufical found.

The tube muft be elaftic;

and the flame from hidrogen.

* Journal de Phyfique, LV. 165.

† Dr. Higgins difcovered it. See Phil. Journal, new feries, I.

a current

a current of hidrogen; for an inflamed ſtream of the vapours of alcohol or ether, a lighted taper, &c. are incapable of producing any ſound.

Circumſtances.

The place of combuſtion is the ſonorous point.

Let us now examine the circumſtances of the experiment. There muſt be a point which may be called the *ſonorous point;* at which the vibrations, communicating an undulatory motion to the air, are produced. This point is the place of combuſtion; for if this be made to alter its ſituation, the ſounds will vary, as M. Pictet has proved in a ſeries of experiments. He has likewiſe obſerved, by means of ſmoke with which he filled the tube, a continual ſucceſſion of vibrations at this point; theſe vibrations produce the waves, which proceeding with a determinate velocity, ſtrike againſt the ſides of the tube, and are reflected with a velocity equal to that with which they arrived.

Undulations at the point muſt accord with the reſonant echo of the tube.

The ſound does not iſſue at firſt.

Its tone.

When the diſtance of the ſides of the tube is ſuch that the alternate reflections are equal to the vibrations natural to the ſonorous cauſe, the ſound increaſes in intenſity, and becomes muſically appreciable. It appears likewiſe that the waves thus reflected do re-act on the primitive vibrations produced at the place of combuſtion, ſo as to render them harmoniouſly regular. For a certain ſpace of time is almoſt always neceſſary before the inſtrument acquires a regular and full ſound; and the tone or pitch of the tube will be more or leſs acute according to the greater or leſs number of undulations which take place in a given time.

The temperature of the air in the tube is hotteſt at the burning point, and cooler more remote.

Alcohol or ether give a leſs degree of heat.

There is another fact eſſential to be obſerved in this experiment. The temperature of the column of air is not the ſame through all its length. At the ſonorous point, that is to ſay the place of combuſtion, the temperature is very high; ſo much ſo that the end of the adjutage of the glaſs through which the hidrogen paſſes, is conſtantly in a ſtate of ignition. If an inflamed ſtream of the vapours of alcohol or ether be ſubſtituted inſtead of the hidrogen, the heat is evidently leſs. From ſome experiments it alſo ſeems probable that the temperature of the room, and the purity of its air, may affect the ſucceſs of the experiment.

Conjecture that the ſound is produced by the rapid production and collapſion of aqueous vapour.

My experiments have been directed to aſcertain the cauſe of theſe phenomena, how and by what means theſe ſonorous vibrations are produced. We know that water is formed during the combuſtion of hidrogen: this water appears in the form of vapour. The temperature of the place of combuſtion being

being very elevated, these vapours must occupy an extensive space; but by immediately coming into contact with a cold air, their volume must rapidly diminish. A vacuum must thus be formed into which the air must collapse, and be again driven by other vapours, which again contract in their turn. Do not the vibrations result from these alternate motions, produced by the great expansion and subsequent contraction of the vapours *? Such were the conjectures that might be formed as to the probable cause of the phenomenon; but accident has presented me a fact which appears to give them some weight.

I had a thermometer tube of about one line in diameter, at the extremity of which was a small bulb: there was a drop of water which I wished to expel from it; for this purpose I repeatedly exposed the bulb to the flame of a lamp with alcohol, and was agreeably surprised to hear a musical sound proceed from the tube. (Sound produced by heating water in a thermometer bulb and tube.)

In order that this experiment may succeed, a tube of one, two, or three lines in diameter, should be taken, of three, four, or five inches in length, with a bulb at one of the extremities, in diameter about three times that of the tube; it need not be very regular. If it were even a little flattened, I think the souud would be louder. A very small quantity of water or mercury must then be introduced, and expose it to a strong (Instructions for making the experiment.)

* It appears probable to me, that the sound produced by air precipitating itself into a vacuum, is more intense than that resulting from an expansive force. The horrible noise occasioned by the detonation of soap bubbles of hidrogen and oxigen is well known, though the lightest objects surrounding the bason are not even shaken. Whence it may be concluded, that the phenomenon is produced by the sudden vacuum proceeding from the destruction of the gases. The detonation of a pistol with inflammable gas is much greater than that of an air-gun, though the effect is less considerable; probably because a vacuum in the pistol succeeds the first expansive force. The child's toy, called the *humming top*, is well known. It is a hollow sphere, with an opening at its circumference; it produces a very strong humming noise when spinned rapidly on its axis. What may be the cause of this humming? I think it is the same as that which I have just mentioned; the centrifugal force drives the air from the sphere through the opening; a kind of vacuum is made, into which the external air continually presses, and is constantly driven back; whence proceed a series of sonorous oscillations.—D. (The sound of air falling into a vacuum much more intense than that from explosion.) (The humming top.)

heat

heat; that of a lamp with alcohol is commonly ſufficient; but the flame muſt be large and ſtrong when the tube is large. After the tube has been thus expoſed for a few ſeconds, the ſound is heard. Thoſe tubes whoſe diameters are conſiderable give a deep ſound, and the ſize of the bulb likewiſe ſeems alſo to contribute to the ſame effect: the ſound continues for ſeveral ſeconds, and then gradually decays, and at length entirely ceaſes. By ſuffering the apparatus to cool, and cauſing the condenſed liquid to deſcend into the bulb, the experiment may be repeated as often as ſhall be thought neceſſary.

Inveſtigation of both kinds of ſound.

Such is the experiment by means of which I think the phenomenon of the muſical tubes may be ſatisfactorily explained. Let us firſt examine what paſſes in the tubes with bulbs, with the eſſential conditions under which they produce their ſound, and then endeavour to diſcover the cauſe. I will then compare it with the ſound produced in tubes with hidrogen, and enquire in what reſpects the effects produced by theſe two inſtruments reſemble one another, with the differences they preſent, and the cauſe of thoſe differences.

Conditions for the tube and bulb.

There muſt be a bulb;

containing water;

The conditions eſſential to the production of ſound in tubes with bulbs are, *1ſt*, That the tube ſhould have a bulb. I never could excite ſonorous vibrations in a tube ſimply cloſed at one end. *2d*, This bulb muſt contain an evapourable liquid. Water ſucceeds very well, but it has this diſadvantage; it forms, in paſſing from the vapourous to the liquid ſtate, a drop of water in the tube, which often completely obſtructs it; and at other times, by running along the warm part of the glaſs, it frequently breaks it. Mercury has not this inconvenience: I never could ſucceed in producing ſounds with ether, alcohol, or concentrated ſulphuric acid. The ſounds are influenced by the quantity of liquid contained in the bulb. It ſhould be as ſmall as poſſible. If there be too much, the vapour fills the tube, and by completely expelling the air, renders it equally warm throughout, and no ſound can then be produced. *3d*, The third condition eſſential to the ſucceſs of this experiment, is the application of a ſtrong heat to the bulb while the reſt of the tube continues cold; for if there be not a marked difference in the temperatures of the tube and of the bulb, no ſonorous effect will take place. *4th*, The preſence of atmoſpheric air in the tube is indiſpenſable; if it be entirely excluded, no effect will be produced; and in every ſtage of the experiment it

or rather mercury; but not ether, alcohol, or ſulph. acid.

The fluid muſt be very ſmall in quantity.

The heat muſt be ſtrong, to the bulb; and the tube kept cold.

There muſt be ſome air in the tube.

it will be found that the vapour occupies only a certain part of the inftrument, and that air is always prefent. I have frequently endeavoured to determine the exact fpace occupied by the vapour at the moment the found was heard, and I found, at leaft in the fmall tubes, that it was nearly equal to the volume of the bulb. In order to determine this, I clofed the orifice of the tube with my finger at the moment it began to found; I then plunged the end of the tube in mercury, withdrew my finger, and fuffered the apparatus to cool. The vapour thus became condenfed, and the fpace it occupied was eftimated by the quantity of mercury which the preffure of the atmofphere caufed to rife in the tube. Proof of this.

Such are the four conditions effential to the production of founds; namely, a bulb at the extremity of the tube; the prefence of a very fmall quantity of water or mercury in this bulb; the application of a ftrong heat to it, whilft the remainder of the tube remains cold; and, laftly, the fimultaneous prefence of atmofpheric air and vapour in the apparatus; it is fcarcely neceffary to add, that the orifice of the tube fhould be always open. Let us now confider what may be the caufe of the found. I firft endeavoured to determine whether any chemical decompofition of the liquid employed took place. For this purpofe I ufed a tube fufficiently long to permit all the liquid to condenfe in it; I weighed it carefully before I fubjected it to experiment, and found that its weight was neither augmented nor diminifhed after repeated productions of found: Whence I conclude, that the caloric has no chemical effect on the liquid, but that it only experiences fucceffive evaporations and condenfations. Muft we then attribute the found to the evaporation and condenfation of the liquid? I thought fo at firft, but the following confiderations made me alter my opinion: I firft obferved that it was poffible to have a fucceffive evaporation and condenfation of the liquid, without producing found: this was effected by applying heat not fufficiently intenfe to produce found. Secondly, When the experiment was made with a drop of water, I always found that the moment when the apparatus began to found, was that at which the whole fluid had evaporated, and confequently that the caloric acts on the vapour alone. While the fmalleft atom of liquid water remained in the bulb, no found was produced. From this fact I have deduced, that the found is produced by the action

Recapitulation.

The found is not produced by chemical change;

nor by evaporation and condenfation;

for thefe laft may happen without found;

action of caloric on the vapour and its re-action on the atmospheric air. I conceive that this phenomenon takes place in the following manner: The vapour contained in the bulb, by means of an addition of caloric applied on all fides and in great quantity, acquires an augmentation of volume and elafticity; it rapidly paffes from the bulb into the tube, and drives out the air contained in it; but this air and the fides of the tube caufe it again to lofe a portion of its caloric, and its volume immediately diminifhes. A vacuum is thus made, and the air refumes its primitive fituation. Another addition of caloric reftores its elafticity, which it fpeedily lofes in the fame manner. A feries of ofcillations by this procefs give the air an undulating motion; and the waves being reflected by the fides of the tube, become fonorous and appreciable as foon as their progrefs and recefs are ifochronous with the ofcillations produced by the caufe here pointed out. There are fome tubes which never produce any found: In this cafe I prefume that the advance and recefs of the waves cannot harmonize with the primitive ofcillations, but that the one deftroys the other. After a certain time the found diminifhes and dies away: This is explained by the propagation of the heat along the fides of the tube when the bulb is very warm and the tube cold. The vapour when firft driven from the bulb, *fuddenly* lofes a part of its volume, and the ofcillations thus produced are ftrong and frequent; but when the tube has acquired a certain degree of heat, the vapour then *gradually* diminifhes in volume as it paffes from a very high temperature to a fituation certainly lefs warm, though of a fufficient degree of heat to caufe the ofcillations to become weaker, and at length entirely to ceafe. That this is the caufe of the ceffation of the found, may be fhewn by applying a ftrong heat to the part of the tube already warmed, and at the fame time keeping up the firft degree of heat at the bulb. By this means the limit of temperature is again precifely determined, and the found reproduced in all its force. It may eafily be conceived that the fubftance of the tube ought to be a non-conductor of heat, and on this account glafs is preferable to any other fubftance.

but to a tremulous expanfion and contraction of the vapour; which thus ftrikes the air and produces found.

It feems as if the expanfions of the vapour ought to be ifochronous with the pitch or action of the tube governed by its length and diameter.

Thefe founds ceafe as foon as the tube has become too hot to occafion the fudden contraction of the vapour.

Experiments in confirmation.

Let us now compare the tubes which found by means of hidrogen with thofe with bulbs. In the former we have every condition neceffary to the production of found; a very hot vapour, and confequently very elaftic; for as we obferved, the

Concerning the founds produced by hidrogen. Repetition of the theory already ftated.

the period of combuſtion is at ſo high a temperature, that it always ignites the tip of the glaſs tube: this hot elaſtic vapour, at the moment of its production, comes in contact with the cold air, which enters through the bottom of the tube and goes out at the top. Its volume muſt be diminiſhed the inſtant it ſtrikes this air; other heated vapours ſucceed the former, and are alſo contracted: this alternate expanſion and contraction produces the undulatory motion of the air, and cauſes the ſonorous waves.

Expanſions and collapſions of aqueous vapour near the point of combuſtion.

We have found that an inflamed ſtream of alcohol or ether produced no ſound in a tube. This is a proof of my obſervation, that in order to produce ſound, there muſt be a great difference in the temperature of the vapour and the ambient air. There is certainly, in this caſe, a formation of vapours and ſucceſſive condenſations, for the water runs along the ſides of the tube; but the point of combuſtion is infinitely leſs hot than that of hidrogen, conſequently the vapour produced is much leſs heated and elaſtic. This caſe is ſimilar to that formerly mentioned, where it was ſhewn that vaporization and ſucceſſive condenſation of the liquid might be effected without ſonorous undulation, merely by expoſing the bulb to a certain degree of heat not intenſe enough for this laſt purpoſe. We cannot be ſurpriſed that there is leſs heat produced by the combuſtion of alcohol or ether than by that of hidrogen, if we conſider, that in the latter caſe all the caloric contained in the gas and the atmoſpheric oxigen which is conſumed, becomes ſenſible heat, and is paſſed totally into the vapour produced. On the contrary, in the combuſtion of an inflammable ſubſtance, ſuch as alcohol, the caloric of the oxigen conſumed alone becomes ſenſible, and this alſo is moſtly abſorbed by the production of carbonic acid gas; ſo that the exceſs alone paſſes into the vapour. It is not therefore ſurpriſing that the heat is not ſufficient to give the vapour elaſticity ſufficient to produce ſounds. The preſence of carbonic acid gas, which is one of the reſults of the combuſtion, may alſo prevent the ſonorous vibrations.

Alcohol and ether burn with leſs heat, and produce elaſtic fluid which is neither rapidly nor totally condenſed.

Why there is leſs heat.

The ſound is much ſtronger in the tubes with hidrogen, than in thoſe with bulbs. It is likewiſe more permanent; for the tube being open at both ends, a current of cold air conſtantly enters at the bottom and iſſues out at the top: this current of air takes up and carries off the hot elaſtic vapours, receives their impulſe, and by attracting a portion of their caloric, diminiſhes

The ſound by hidrogen is ſtrong and permanent, becauſe the conſtant ſupply of cold air produces the condenſations inceſſantly.

minishes their volume. This then is the most essential condition to the production of an intense permanent sound; namely, a great difference between the temperature of the air and the vapour; and this difference remains always the same, on account of the continual renewing of the air. The same effect does not take place in the tubes with bulbs, and accordingly their sound is weaker and less durable.

Not so in the bulbs.

The sounds by hidrogen are less intense in a warm room filled with company; because the air is warmer, and the combustion weaker for want of oxigen.

From the knowledge of this condition, that a great difference between the temperature of the air and the vapour is necessary to the production of sound, it will be easy to comprehend that every circumstance which augments the heat of the current of air, and diminishes that produced by the combustion of the gas, will tend to weaken and even to annihilate the sound of the tube. These two circumstances are both found in a warm room filled with company. The current of air instead of being cold is heated, and the quantity of oxigen being less, the heat of the combustion is less. It is not then surprising that in such rooms the experiment does not always succeed.

Sounds by phosphorus.

Brugnatelli produced sounds by the simple combustion of phosphorus in tubes. Some philosophers being persuaded that sonorous effects could only be attributed to hidrogen, were disposed to infer its presence in phosphorus. From what we have stated, it seems more simple to explain the phenomenon by the production of phosphoric acid in the vaporous form, which becomes strongly elastic by means of the caloric disengaged during the combustion, but of which the volume diminishes by the contact of cold air. We have here the alternate expansion and contraction necessary to the production of sound.

Conclusion.

Such are the small number of observations which I have had an opportunity to make on the sonorous tubes. I hope that they will interest such as are more particularly engaged in this branch of natural philosophy, and tend to direct their attention to a curious fact which has not hitherto been sufficiently examined.

ANNOTATION.—W. N.

That aqueous vapour is condensed with great rapidity;

AS the fundamental effect in this very ingenious explanation (namely, that aqueous vapour is condensed with extreme rapidity), may not appear to some readers to be so

so speedy as to produce sonorous undulation, I think it will not be impertinent to mention a few facts relating to that subject. In the small glass instrument called the water-hammer, which is a tube of about three quarters of an inch in diameter, with a ball of about an inch and half at one end, the other end being hermetically closed; the ball contains water, and the empty space is rendered nearly vacuous by boiling the fluid previous to sealing it. In this instrument the heat of the hand, applied to the wetted tube, is sufficient to produce bubbles of vapour which enter the ball, but speedily collapse. The series of these condensations is as quick as fifteen or sixteen in a second. But in the steam engine the condensation is prodigiously more rapid. I have a small double steam engine on the construction of Boulton and Watt, having all the parts and gear of the large engines, but its cylinder is only 2½ inches diameter, and the length of stroke 6¼ inches. When this was set to work the other day, in a lecture to my pupils, it gave 600 * strokes per minute. By an easy calculation it will be shewn, that the steam condensed was then much more than 300 cubic inches per second; and if the condensation, instead of being effected in masses of about a pint at a time, could have been performed by successive collapsions of each cubic inch in an open space, the pulses would have produced the tone of the lowest E flat in the treble cliff. But the number of cubic inches condensed in a large steam engine, for example, a three feet cylinder with an eight feet stroke, will be eight or nine times as much, at the usual rate of working.

shewn by the water-hammer,

and by the steam engine.

A small steam engine working twice as quick as the balance of a watch; and condensing ten pints of steam in a second.

This condensation, if by successive inches, would give a very loud E flat.

* About twice as many as the beats of a common watch.

V. Account

V.

Account of a simple Method of estimating the Changes of Volume produced in Gases, by Alterations of Temperature, and of Atmospheric Pressure, in the course of Chemical Experiments. By MR. H. DAVY. *Professor of Chemistry at the Royal Institution. From their Journal.*

Elastic fluids affected by pressure and temperature.

IT often happens that changes of atmospherical pressure, and of temperature, take place in the course of experiments on elastic fluids; and a knowledge of the alterations they produce in their volume, is essential to the precision and accuracy of the results.

Quantities of gases must be estimated at some standard.

In cases when chemical changes are produced, it is impossible to gain this knowledge by direct observation; and, in considering quanties, it is always useful to estimate the volumes of gases at some standard, fixed upon in measures of the barometer* and thermometer.

Law for pressure. Volume, inversely as pressure.

It is demonstrated by very accurate experiments, that the volumes of elastic fluids are inversely as the weights compressing them. And, consequently, the changes produced upon gases, by known changes in the atmospherical pressure, may be ascertained in a very easy manner. With regard to the effects of temperature, however, it is much more difficult to form a just estimation by means of general laws. For though the excellent experiments of Mr. Dalton †, and those of

* Lavoisier's Elements, p. 406, 2nd Edit.

† Manchester Memoirs, Vol. V. p. 599. Mr. Dalton says, "I have repeatedly found that 1000 parts of common air, of the temperature of 55° and common pressure, expand to 1321 parts in the manometer;" to which adding 4 parts for the corresponding expansion of glass, we have 325 parts increase upon 1000 from 55° to 212°; or for 157° of the thermometric scale. As for the expansion in the intermediate degrees, which Col. Roi's experiments show to be a *slowly diminishing* one above the temperature of 57°, but which de Morveau's, on the contrary, show to be a *rapidly increasing* one in the higher part of the scale; I am obliged to allow that Col. Roi is right, though it makes, in some degree, against an hypothesis I have formed relative to the subject; he has certainly, however, made the diminution too great from 72° downwards, owing

of Mr. Gay Lussac, show that all the different elastic fluids, taken at equal temperatures, expand equally by heat, yet we are still ignorant of the precise expansion for increments of single degrees, or of the mode in which the power of expansion is affected by difference of pressure; or by its relations to aqueous vapour or uncombined moisture.

The calculations on this subject are consequently laborious and complicated; but it is very easy to avoid them, by recurring to comparative observations, which may be obtained in a very simple manner, by means of the manometer. Calculation may be avoided by comparative observations,

By making a given quantity of air, in contact with water at a known temperature and pressure, a standard, it is easy, in ascertaining the changes produced in it by alteration in the temperature and pressure, to determine, by the rule of proportion, the changes that have been produced from the same causes in any other quantity of gas, submitted to chemical operation; or to ascertain what would be its volume at the mean height of the barometer and thermometer. with a standard quantity of common air,

owing to his not perceiving that he actually destroyed a portion of the elastic fluid he was operating upon (aqueous vapour) in reducing its temperature so low; if his air had been previously dried by sulphuric acid, &c. he would not have found so remarkable diminution below 72°. My experiments give for 77°½ above 55°, 167 parts; for the next 77°½ only 158 parts: and the expansion in every part of the scale seems to be a gradually diminishing one in ascending.

"The results of several experiments made upon hydrogenous gas, oxygenous gas, carbonic acid gas, and nitrous gas, which were all the kinds I tried, agreed with those on common air, not only in the total expansion, but in the gradual diminution of it in ascending: the small differences observed never exceeded 6 or 8 parts on the whole 325: and differences to this amount will take place in common air, when not freed from aqueous vapour, which was the situation of all my factitious gases.

"Upon the whole therefore I see no sufficient reason why we may not conclude, that *all elastic fluids under the same pressure expand equally by heat*—and that *for any given expansion of mercury, the corresponding expansion of air is proportionally something less, the higher the temperature.*"

Mr. Gay Lussac makes the dilatation to be from 100 to 137.5 between 32° and 212°, which gives for each degree between these points $\frac{1}{493}$. See p. 134; and Annales de Chimie, No. 128, p. 137.

confined in a recurved tube by water in the bend; In forming the manometer for the purpofe of comparifon, a glafs tube may be ufed about fixteen inches long, and one third of an inch in diameter, clofed at one end, and curved in fuch a manner that its open leg is parallel to its clofed end, and nearly three inches long. Its capacity is determined, and its clofed leg is graduated fo as to form a fcale of 200 parts. The ftandard volume of air is confined in it by a column of water, four inches long; and the height of this water is kept equal in both legs at the different times of obfervation, by means of a glafs tube, moveable in a perforated cork inferted in the open end, and capable of elevating the column of water in it at leaft an inch and a half.

graduated

the water kept level, by a plunger.

Ufe of the ftandard of comparifon. In employing the ftandard of comparifon, for correcting the refults of operations in which portions of elaftic fluids are either abforbed or generated, it is only neceffary to recur to the ftate of it at the beginning and end of the experiment.

Inftance. Thus, let n equal the quantity of elaftic fluid exifting after the experiment; m the volume of the ftandard air before the experiment; and v the volume after, as expreffed in the fcale of 200 parts.

Then $\frac{nm}{v} = x$, which is the volume the refidual gas examined would occupy at a temperature and preffure fuch as exifted at the commencement of the experiment.

By the fame method may be eftimated the volume a quantity would occupy at fuch temperatures and preffures, as had been at any time denoted upon the fcale.

It is moft probable that the refults are not difturbed by the aqueous vapour. From the lateft experiments that have been made *, it is probable that at the fame temperatures, and under the fame preffures, equal volumes of the different elaftic fluids, in contact with water, contain the fame quantity of aqueous vapour; fo that in cafes when the gafes examined in the comparative obfervations are equally faturated with water, the refults muft be perfectly accurate as to the relation of volume to the ftate of moifture; and, even fuppofing a difference in the degree of faturation, the error arifing from this circumftance, at common temperatures, would be fo fmall as to be inappreciable.

* Thofe of Sauffure and De Luc, and MM. Desormes and Clement.

VI. *Obfervations*

VI.

Observations on Sir George Shuckburgh Evelyn's Paper * *in the Philosophical Transactions for* 1798, *on the Standard of Weight and Measure. By* J. FLETCHER, *Esq. Communicated by the Author.*

I. HAVING about two years ago had occasion, in the course of some investigations in which I was then engaged, to ascertain with as much precision as was possible the relation between the known measures of distilled water and their respective weights at different temperatures; Sir George Shuckburgh Evelyn's Paper on this subject was of course considered as the best authority with regard to what had been previously done in this respect. I found however, on looking into it, some errors in the calculations, which render the deductions from them incorrect, at the same time that they more strongly confirm and establish the accuracy of the experiments on which they are founded. I have no doubt, therefore, that the gentleman to whom we are so highly indebted for the communication of the facts which he states in this important essay, will feel rather obliged than otherwise by their correction. The circumstance which first induced me to repeat the calculations, was the difference which there appears to be in the results of the experiments with the cube and sphere, and that with the cylinder, and which Sir George ascribes, in § 24 and 31 of his Paper, to either a difference in the density of the water at different depths, or to the compressibility of the cylinder employed. I was however as much pleased as surprized to find, that it altogether arises from a mistake in the multiplication of the four numbers whose product is equal to its solid content, *viz.* 3.99745 × 3.99785 × 5.99502 × .78539816, which he makes 74.94823 cubic inches, instead of 75.247149, which is the real product. This naturally suggested the propriety of examining the whole, and the results of this examination are what I shall now lay before the public through the medium of the Philosophical Journal. The power of correctly appreciating and comparing space and quantity are so essential to

Occasion of this essay.

Numerical errors in Sir G. S. Evelyn's paper.

Apparent difference in results.

Mistake in the solid content of his cylinder.

* Copied in the Quarto Series of this Journal, III. 197.

 every

every practical application of the mathematics to the purposes of real utility, that I have no hesitation in undertaking the humble office of commentator on so valuable a text.

Real cubic dimensions of Sir G. S. Evelyn's solids.

II. It is stated in § 13* that the product of the three mean measures of the sides of the cube, *viz.* $4.98882 \times 4.98955 \times 4.98925$, is 124.18917: this however is erroneous, it being in fact 124.192246. The solid content of the cylinder was 75.247149, as already stated in the preceding paragraph, and that of the sphere (§ 28) $= \overline{6.00745}^3 \times .52359878 = 113.519147$ cubic inches.

Correction for temperature of the brass.

III. Supposing the brass scale and these bodies to be at the same degree of heat, their apparent dimensions as measured by it would be the same whatever was the temperature of both; the scale being of the same substance, and consequently expanding and contracting with the bodies themselves. Their real dimensions however varying with the temperature, we must assume some certain degree of heat at which the graduations on the scale are to be considered as measuring correspondent intervals of space. We will suppose this to be 60° of Fahrenheit's thermometer. Now in order to ascertain the real cubic measure by the scale when at 60°, of the quantities of distilled water displaced by those bodies when they were weighed in it, we must add to or subtract from the nominal cubic dimensions of each, 3 millionth parts (vide Tab. I. in the note on § 23), multiplied by the number of degrees above or below 60°, at which they and the fluid were at each of those times.

At the time of their being respectively weighed in water, therefore, the solid content of the cube at 60.2° (§ 22) was $124.192246 \times 1.00000062 = 124.192323$ cubic inches of the scale when at 60°; that of the cylinder at 60.5° (second experiment, § 23) $75.247149 \times 1.0000015 = 75.247161$; and that of the sphere at 66° (§ 29) $113.519147 \times 1.000019 = 113.521301$.

Correction necessary for density and temperature of the air.

IV. In appreciating the apparent difference of weight in a body when weighed in air or in vacuo, it has been usual to assume, that air at a given temperature, and under a given

* These references with a § prefixed, are to the sections so numbered in Sir G. S. Evelyn's Paper, which it is thought proper to mention here to save the necessity of repetition.

pressure is always of the same specific gravity. This we know is not the case; considerable variations in this respect being occasioned by its different states of dryness, electricity, and perhaps other causes: as, however, experiments are wanting for the correct estimation of these differences, we will take the first supposition to be true, and apply the corrections accordingly from Sir G. S. Evelyn's tables for this purpose, in his note subjoined to § 23.

This gentleman tells us in the Philosophical Transactions for 1777, that the specific gravity of air at 51°, under a pressure equal to 29.27 inches of mercury, is to that of water as 1 to 836, or = .001208; taking the latter as unity. Assuming therefore that the expansion of air, for each degree of Fahrenheit's thermometer between 51° and 60°, is .0027 of the whole, we get the specific gravity of air at 60 under the like pressure = .001179, and 29.27 : .001179 : : 29.5 : .001188 = the specific gravity of air at 60° when the barometer stands at $29\frac{1}{2}$; and a cubic inch of such air consequently weighs $\frac{3}{10}$ of a grain very nearly.

Air expands .0027 for 1° of Fahrenheit.

Sp. gr. of air at 60°, bar. 29.5, = .001188 = .3 gr. for each cubic inch.

Now the cube was weighed in air at 62° under a pressure of 29 inches (§ 20); the cylinder in air at 62° under a pressure of 29 inches (§ 21); and the sphere in air at 67° under a pressure of 29.74 inches (§ 29).

Densities of the air in which the bodies were weighed.

And the excesses of the weights of the respective quantities of distilled water displaced by these bodies, on their being afterwards weighed in this fluid, over those of similar quantities of air at the several temperatures and densities above-mentioned, were as follow; *viz.*

That of the water displaced by the cube (§ 22), 32084.82 − 703.03 = 31381.79 grains;

The apparent weights of equal bulks of water.

That of the water displaced by the cylinder (second experiment § 23), 21560.05 − 2553.22 = 19006.83 grains;

And that of the water displaced by the sphere (§ 30), 28673.51 grains.

The air in which each of the bodies was weighed being, however, lighter than air at 60° when the barometer stands at $29\frac{1}{2}$, we must correct these excesses of weight in the several quantities of water, so as to find how much the weight of each of them exceeds that of an equal quantity of air of such standard specific gravity; or in other words, how much the weight of each of the bodies in water differs from that which

Correction for pressure and temperature applied.

it

it would have been found to poffefs in air in this latter ftate. Thefe corrections for the denfity and temperature of the air we fhall find, from the data before given in this article, to be as follows:

	Correction for	
	Preffure.	Temperature.
	Gr.	Gr.
Of the weight of the water difplaced by the cube - - -	− .642	− .201
Of the weight of the water difplaced by the cylinder - - -	− .389	− .122
Of the weight of the water difplaced by the fphere - - -	+ .282	− .644

Correction of fp. gr. of water when reduced to 60°.

The weights of thefe bulks of water, therefore, if weighed in air at 60° under a preffure of $29\frac{1}{2}$, would be found to be,

That difplaced by the cube 31381.79 − .642 − .201 = 31380.95 grains;

That difplaced by the cylinder 19006.83 − .389 − .122 = 19006.32 grains;

And that difplaced by the fphere 28673.51 + .282 − .644 = 28673.15 grains.

V. We have now, however, to correct thefe weights for the change which would take place in the fpecific gravity of the water itfelf, by reducing it to 60° of temperature; that in which the cube was weighed having been at 60.2° (§ 22); that in which the cylinder was weighed at 60.5° (§ 23); and that in which the fphere was weighed at 66° (§ 30).

Firftly, by Sir G. S. Evelyn's table.

We will firftly do this by the help of Sir G. S. Evelyn's Tab. I. in his note on § 23, according to which, a quantity of water of equal bulk with the cube would, at 60° of Fahrenheit's thermometer, weigh 31380.95 × 1.000033 = 31381.986 grains; one of equal bulk with the cylinder, 19006.32 × 1.000083 = 19007.897 grains; and one of equal bulk with the fphere, 28673.15 × 1.00099 = 28701.536 grains; if weighed in air at the fame temperature, when the barometer ftands at 29 inches and a half,

The

Result.

	Grains by Sir G. S. Evelyn's Weights, or	Grains of the Parliamentary Standard Weights, which are those used by him as 1.0006562 : 1. (§ 41).
The first gives the weight of a cubic inch of distilled water under these circumstances $= \frac{31381.986}{124.192323} =$	252.689	252.523
The 2d $= \frac{19007.897}{75.247181} =$	252.606	252.440
And the 3d $= \frac{28701.536}{113.521304} =$	252.829	252.663

VI. It may perhaps however be proper to observe here, that the alteration in the specific gravity of water by change of temperature, is estimated in this table of Sir G. S. Evelyn's (note on § 23), at nearly double that which is given by Mr. Gilpin in his tables, in the Philosophical Transactions for 1794, p. 382; his appreciation of it from 60° to 66° (taking it to be equal to unity at 60°) being as follows; *viz.*

By Mr. Gilpin's tables;

61°	.99991	63°	.99971	65°	.99950
62	.99981	64	.99961	66	.99939.

I am, from my own experiments on this subject, disposed to prefer these tables; and taking them to be correct, we shall have a cubic inch by Sir G. S. Evelyn's scale when at the temperature of 60°, of distilled water also at 60°, weighed in air at 60°, under a pressure of 29½ inches.

Ultimate result. A cubic inch of water at 60°, in air at 60°, bar. 29.5, = 252.506 grains;

By the cube = - -	252.519	Grains of the Parliamentary Standard according to Sir G. S. Evelyn's appreciation of it in § 41 of his Paper.
By the cylinder = - -	252.432	
By the sphere = - -	252.568	
And by a mean of all three =	252.506	
And if weighed in vacuo instead of in air = - -	252.806	

or = 252.806 grs. in vacuo.

J. FLETCHER.

Cecil Street, Dec. 20, 1802.

VII. *On*

VII.

On the Quantities of Light afforded by Candles in Proportion to the Consumption of Material and other Objects respecting the same. In a Letter from MR. EZ. WALKER.

To Mr. NICHOLSON.

SIR,

Probability that Candles which burn without producing smoke, will afford light in proportion to the matter consumed.

WHEN a lighted candle is so placed as neither to require snuffing nor produce smoke, it is reasonable to conclude that the whole of the combustible matter which is consumed is converted to the purpose of generating light; and that the intensities of light generated in a given time by candles of different dimensions, are directly as the quantities of matter consumed. That is to say; when candles are made of the same materials, if one candle produce twice as much light as another, the former will in the same time lose twice as much weight as the latter.

Experiments in proof.

To prove the truth of this position, I made the experiments contained in the following

TABLE.

No. of the Experiment.	No. of the Candles.	Time of burning.	Weight of the Candles consumed.	Strength of Light.	Distance of the Candles from the Wall.
		h. ′	oz. dr.		Feet.
1	1	3 0	0 15	1.	7
	3	3 0	1 1½	1. +	7
	Mould	3 0	0 15	1.	7
2	1	2 55	0 15	1	8
	3	2 55	1 0	1 +	8
	Mould	2 55	0 15	1	8
2	1	3 0	0 15¾	1	8
	3	3 0	1 2	1⅛	8¾
	Mould	3 0	1 0	1	8
4	5	3 0	1 5	1.18	8¾
	Mould	3 0	1 1⅛	1.	8

These

These experiments were made in the following manner.

I took three candles, the dimenſions of which are given in the table, againſt 1, 3, and mould *. Theſe were firſt weighed, and then lighted at the ſame inſtant. At the end of the time inſerted in the third column of the above table, they were extinguiſhed and weighed again, and the loſs of weight of each candle is contained in the fourth column.

Method of experiment.

The three firſt experiments were made under ſuch favourable circumſtances, that I have little doubt of their reſults being more accurate than what practical utility requires, but the fourth experiment cannot be depended on ſo much, in conſequence of the variable light of No. 5. This candle was moved ſo often to keep the two ſhadows equal, that I was under the neceſſity of ſetting down its mean diſtance from the wall by eſtimation; but as this was done before the candles were weighed, my mind could not be under the influence of partiality for a ſyſtem.

Obſervations on the reſults.

The method which I uſed in comparing one light with another in each experiment, was nearly that which has been already deſcribed †. The only difference conſiſted in having the obſtacle which formed the ſhadows fixed, inſtead of being held in the hand. A two feet navigation ſcale, which was made faſt to the ſtand of a ſmall teleſcope in a perpendicular direction, and ſet upon a table near the wall on which the ſhadows were compared, reduced the labour of making the experiments very much, and gave me an opportunity of making a greater number of obſervations in the ſame time than I could have done with the ſcale in my hand.

Intenſity of light determined by the method of ſhadows.

REMARKS ON THE EXPERIMENTS.

The ſtandard compariſon of experiments at different times was a mould candle by the weight conſumed, of which the lights were infered.

1. The experiments were made at different times, and the light of the mould candle was made the ſtandard, with which the lights of the others were compared; but it muſt not be underſtood, that this candle gave the ſame ſtrength of light in every experiment.

2. The ſign + in the 5th column, ſignifies that the candle againſt which it is placed, gave a ſtronger light than the others

Other remarks.

* Philoſophical Journal, octavo, III. 273.

† Philoſophical Journal, quarto, I. 67. and Philoſ. Journal, octavo, III. 275.

in

in the ſame experiment, but the exact quantity being ſmall, I did not aſcertain by calculation.

3. The mould candle in the 4th experiment, loſt more of its weight in three hours, than in the preceding experiments. This was owing to the current of air in the room in which the experiments were made, being greater than uſual, occaſioned by a freſh gale of wind.

GENERAL LAWS.

General law as at firſt ſtated.

From the experiments contained in the table, it appears to be an eſtabliſhed law, where combuſtion is complete, that the quantities of light produced by tallow candles, are in the complicate ratio of their times of burning and weights of matter conſumed.

For if their quantities of matter be equal, and times of burning the ſame, they will give equal quantities of light, *by the experiments.*

And if the times of burning be equal, the quantities of light will be directly as their weights of matter expended, *by experiments 3 & 4.*

Therefore the light is univerſally in the compound ratio of the time of burning and weight of matter conſumed.

STANDARD OF LIGHT.

More ample explanation reſpecting the ſtandard candle.

If the law which I have endeavoured to prove, both by reaſon and experiment, be admitted, we have a ſtandard by which we may compare the ſtrength of any other light.

Let a ſmall mould candle, when lighted, be ſo placed as neither to produce ſmoke nor require ſnuffing, and it will loſe an ounce of its weight in three hours. Let this quantity of light produced under theſe circumſtances, be repreſented by 1.00.

Then ſhould this candle at any other time, loſe more or leſs of its weight in three hours than an ounce, the quantity of light will be ſtill known, becauſe the quantity of light in a given time is directly as the weight of the candle conſumed, *by the general law.*

CONCLUSION.

Advantages of the inclined candle;

A candle which is uſed in the manner that I have pointed out, gives *more light* than a candle of the ſame dimenſions ſet

 perpendicularly

perpendicularly and ſnuffed, becauſe one part of a candle that is ſnuffed is thrown away, and another part flies off in the form of ſmoke. But this is not the only inconvenience that attends the uſing of a candle in this manner, and which the other method is free from, for the light which it gives is of a bad quality, on account of its being variable and undulating.

From the time that a candle is ſnuffed till it wants ſnuffing again, its ſtrength of light ſcarcely continues the ſame for a ſingle minute. But that variation which frequently takes place in the height of the flame, is a matter of ſtill more ſerious conſequence. contraſted with the upright candle, which requires ſnuffing.

The flame of a long candle, when it burns ſteadily, is about two inches high, but it very frequently riſes to the height of four inches or upwards; drops down again in a moment, till it is leſs than three inches, and then riſes again. In this manner the flame continues in motion for ſome time before it returns to its original dimenſions. But it does not continue long in a quieſcent ſtate before it begins a new ſeries of undulations. In this manner the candle burns till the top of the wick is ſeen near the apex of the flame, carrying off clouds of ſmoke. In this ſtate of things the eye becomes uneaſy for want of light, and the ſnuffers are applied to remove the inconvenience. Extreme undulations of large candles placed vertically;

It is theſe ſudden changes, and not the nature of candle light itſelf, that do ſo much injury to the eye of the ſtudent and artiſt; but that injury may be eaſily prevented, by laying aſide the ſnuffers, and in the place of one large candle, let two ſmall ones be uſed in the manner which I have before taken the liberty to recommend. and the miſchievous conſequences to the eyes.

I remain,

SIR,

Your's reſpectfully,

EZEKIEL WALKER.

Lynn Regis, Dec. 20, 1802.

Deſcription

VIII.

Defcription of an Engine for raifing and lowering Weights by the Action of a Column of Water and for other purpofes. By John Harriott, *Efq.*

To Mr. NICHOLSON,

Dear SIR,

I SEND the drawings and defcriptions of my fyphon engine, which from its power and great convenience in actual practice, will I prefume be thought a fit fubject for your valuable publication.

Engine acting by a column of water acting alternately on the upper and under furface of a pifton, as directed by turning a cock.

A A. In Fig. 1 and 2, is a cylinder with a moving pifton therein, of which D is the pifton rod.

B and C. Are water ways through which the water is admitted to communicate with both fides of the pifton.

E F. A pipe in Fig. 1, through which water defcends from a refervoir above, into a three way cock M. and in Fig. 2, is a pipe through which any ftream or head of water runs to the three way valve in the ciftern M.

C H. Is a pipe in both, communicating from the three way cock, or valve, to the upper part of the cylinder.

K B. Is a pipe communicating from the fame cock or valve, to the lower part of the cylinder.

I I. Is a pipe communicating between the two laft mentioned pipes, confequently between the upper and lower fpaces of the cylinder, which communication can be either cut off or opened to any requifite degree by the cock L.

N. Is a pipe in which a lower column of water is fufpended by the reaction of the atmofphere, and confequently a power to the upper column, or fall in proportion to its length or depth, not exceeding the weight of the atmofphere.

The column confifts partly of an actually preffing mafs and partly of a fufpended portion, which renders the atmofphere active on the machinery.

The nature and principle of the fyphon engine confifts in combining the power of the fyphon with the direct preffure of a column or ftream of water, fo that they may act together. It works by means of the fyphon conftantly acting in concert with the feeding ftream of water, fo that each alternately act on the upper and lower part of a pifton, within a cylinder as it were, reverfing the fyphon at each change; and the power is equal to a column of water of the fame diameter as that of the

the cylinder, and equal in length to the height of the head, above the tail water. For inſtance, if a column of water of any given diameter has a fall of 20 feet until it reaches an engine, its power is clearly aſcertained. Now whatever that power is, if a ſyphon pipe be added to this engine, ſo as to connect with the column, and the ſyphon pipe has alſo a fall of an equal length; namely, 20 feet to the lower end, which is immerſed in water, the engine although placed in the midway, will then have a power equal to that of a deſcending column of 40 feet, and ſhould the column or fall to the engine be but two feet, and the lower ſyphon pipe 24 feet, the power would be equal to a fall of 26 feet; and in this manner in every various diverſity between the falling column and the ſyphon pipe beneath, the latter will produce an equal power according to its proportionate length, or depth to the ſurface of the tail water, provided it does not exceed above 30 feet, or the weight of the atmoſphere; and where a ſtream of water is either level with or even below, the place at which it is deſirable to fix the engine, there will be no difficulty in placing it either below, or on the level, or above the ſtream itſelf, provided the height where it is fixed above, does not exceed 28 or 30 feet, and the place where the water flows off be ſtill lower. The conſtruction may evidently be varied according to the local ſituation and circumſtances of applying it, and the uſe to which it may be adapted, in giving activity to different kinds of machinery.

The drawing, Fig. 1, exhibits the apparatus for raiſing or lowering weights of any kind, on wharfs or in warehouſes. A man or boy can raiſe or lower goods of any weight, without other exertion than that of merely turning the three way cock M. to an index; in either raiſing or lowering, the *ſtop* is inſtantaneous, by a ſmall motion or turning the cock to the ſtop mark in the index: this moſt effectual of ſtops, or gripe, operates ſo quietly and eaſy without any jirk, or jarring, that it removes the uſual riſk attending common cranes or machinery in which men are ſometimes overpowered. It raiſes and lowers goods with thrice the velocity uſually produced by manual labour, yet an engine of dimenſions ſufficient to raiſe ſeveral tons, may be ſo graduated by the perſon at the cock, as to bring it to the ſmootheſt floweſt motion poſſible. The ſaving of labour and time muſt therefore be conſiderable, the riſk

Engine to be uſed inſtead of a crane. Its advantages ſtated.

risk of plunder diminished, and delays in setting to work for want of help removed.

Though this engine requires a reservoir of water; yet this may easily be had and will be highly beneficial in cases of fire.

The great and only obstacle to its general application in this way, is the want of a natural head of water in most situations of warehouses; and the question in these cases would be, whether it would be worth while to raise a head or reservoir of water for such purposes. If this be done, *one* reservoir would work any number of engines at any distance by one main pipe, and as many branch pipes as there were engines. The expence of a small steam engine to raise the same water (or any other) to the reservoir, would be more than repaid by the security it would give against fire. A gallon of water at the first alarm of fire, has more effect in extinguishing it, than a hogshead after it has got a-head. To produce this effect, and apply it in the speediest manner wherever such reservoir is, would only require small pipes to be led from the main pipe that supplies the engines, to the warehouses, or where it may otherwise be wanted, and at the end of such pipe, a screw nozzle and a cock to turn on or off, a leather hose and branch pipe, such as all fire engines have, being provided and hanging near. One man only at the first alarm of fire, would in a few minutes screw the hose upon the screw nozzle, and then turning the cock, he would of himself be able to play upon the fire with all the force of a strong fire engine.

Proposal for public reservoirs of water in every parish.

Perhaps it is not too much to say, that this ready preventative in case of fire, would be equal in effect to an insurance. Independent therefore of its application to the syphon engine for raising and lowering weights, I have conceived, that if such reservoirs of water were provided in all cities and towns, well elevated at suitable distances (one or more in every parish) there would not be near the danger and calamity from fire that now exists. The distance of the reservoir from the fire is of no consequence, provided the pipes were laid with sufficient water way; and it would seldom happen that more than one fire at a time would break out in the same parish, or if it did, the same speedy and effectual assistance could be given by any number of such powerful and self-working engines, as long as there was water in the reservoir. And even supposing the supply from the reservoir to be limited to any given time of expenditure, common prudence would lead to its being replenished by the usual mode, before it could be entirely exhausted.

The

The drawing Fig. 2, shows how the syphon engine is to be applied to streams of water, the advantages of which are, that the engine as well as the mill work, or manufacturing machinery it may drive, may be placed where most convenient, above or below the head or stream, to be worked by a fall of water from the least to the greatest height, or by any stream or river, the tail water below acting and having as much power as the head, answering to the height of either. Nor can a drop of water escape without performing its full duty. The power is therefore greater, and not liable to the disadvantages attending a water wheel. In tide waters it would work ebb and flood so long as there was a difference of two feet or less in the height, regulating itself, so that the power may be equal, let the head and tail water rise or fall, by which a smooth uniform motion is maintained and adapted to the smallest as well as the strongest power wanted. If the tail water re-acts upon a water wheel, it must lose so much of the power of the fall, or in other words, whenever the tail water rises above the lowest wash boards of the wheel, a counter action will be exerted as is well known against the power of the wheel. A considerable quantity of the stream must likewise escape without any good effect. These disadvantages of the water wheel, are removed by the syphon engine; and in frosty weather being fed by a pipe from below the ice, it will not be impeded, because its velocity in passing through the engine, will prevent the water being exposed a sufficient time to the cold atmosphere to congeal it; and when at rest the engine may be left empty. It is scarce necessary to observe, that when the engine is fixed below the running head of the stream, it will fill as soon as the sluice is opened, and set itself to work; but when placed above the head, it will require sufficient water to fill the whole interior space, which being thus charged and converted into a syphon, will then work as well and with as much power as if the engine were placed below the head of the stream.

Other uses and applications of the engine.

Frosty weather affects it less than mills and other hydraulic machines.

I am,

Dear SIR,

your most obedient and

very humble servant,

JOHN HARRIOTT.

Thames Police, Dec. 20, 1802.

REMARKS.

REMARKS.—W. N.

Confideration of the two principal objections to a water engine inftead of a crane.

The convenience and facilities afforded by Mr. Harriott's Engine, will be obvious to every one; and the only circumftances which can be urged in the way of objection againft it, are thofe which he has himfelf noticed; namely, that it in general requires the water to be raifed, which is to work it as a crane, and that this water is fubject to freeze. To meet thefe difficulties fairly, we muft admit, that if a ton of water were raifed to the top of a warehoufe, in order by its fall to raife a ton of goods (even if it could do that) it would be fimpler for the warehoufeman to raife his goods without the intervention of the fluid. But while we admit this general truth, it muft alfo be obferved, that the pofitions do not include the whole of the actual cafe. The raifing and lowering of goods being a procefs of confiderable fkill and intelligence, and being neceffarily carried on with many ftops, interruptions, and variations of force, is on thefe accounts performed by the moft expenfive of all firft movers; namely, the ftrength of men, and even this power is for a large part of the time inactive; namely, during all the intervals of operation. Here then is a wide field for the faving of force, if it could be ftored up and ufed when wanted. Suppofe for example, that two men were employed in a crane, and that the paufes of inactivity amounted to one third of their whole time, thefe men would be more beneficially employed in raifing water, to be afterwards directed by a boy, or by the foot of a clerk who ftood by, to keep account of the delivery of packages. But as horfe work is reckoned at leaft five times as cheap as human labour, the faving of labour by employing that animal, would be about fix fevenths of the whole; and fteam engine work upon a very fmall fcale indeed, would be as cheap again as horfe work. Hence it appears, that after every allowance for the greater quantity of water required to produce velocity by its fall, and for other circumftances the faving of labour muft be very great, by thus œconomizing and ftoring up the force, exclufive of the conveniences detailed by Mr. Harriott in his letter.

Labour of raifing the Water, compared with that now employed to hoift and lower goods. Men. Horfes. Steam.

The labour faved by Mr. Harriott's engine (fupplied by fteam) would exceed fix fevenths of that now expended.

Froft would moft probably affect it in mill work as it does other engines;

The freezing of the water is an impediment of fuch a nature, that it will perhaps be found that the operations of natural

streams or falls of water would be subject to interruptions from this cause, with his engine about as soon as with others that are more exposed, so that its advantages would consist of the other particulars which he has detailed. But with the engine applied for raising goods, it is to be apprehended that the water in the elevated reservoir would be bound up by freezing, and also in severe weather checked if not confined by the same cause in the engine itself. The remedy for this appears to be to work the engine, by raising the same mass of fluid repeatedly, and dissolving some cheap material in it which should render it less disposed to freeze. Most saline bodies would have this effect, and the quantity once added, would remain long without waste or loss. Experiment would shew what salt, and whether earthy, neutral, metallic, alkaline, or acid, might be the cheapest, most effectual, and least disposed to act upon the engine *.

but in crane-work the water may be rendered incapable of freezing in our climate;

by a moderate addition of some cheap saline substance.

IX.

On the Electricity of the Shavings of Wood. *By Mr.* W. WILSON.

SIR, *London, Dec.* 13, 1802.

YOUR readiness to insert my letter of the 11th of October, encourages me to request the insertion of the following account of experiments on the electricity obtained by cutting of wood, &c. In doing which you will very much oblige

your obedient humble servant,

WM. WILSON.

HAVING frequent occasions to work very dry wood that has lain over a large fire for several hours, I have often observed the shavings, &c. to adhere to the tools, and whatever they touched. About two years ago I began to take particular notice of, and endeavour to find the cause of this phe-

Wood shavings adhere to the tools.

* In the consideration of this engine, the attention of the reader will be directed to the pressure engine of Mr. Trevithick, in our first volume.

nomenon, and after ſatisfying myſelf that it was not occaſioned by any moiſture or roughneſs on the ſubſtance; I ſuſpected that electricity was the cauſe, and I accordingly ſet about the experiments which are the ſubject of this communication.

Dry warm beach ſhaved with glaſs is poſitive,

I laid a circular tin plate, 6 inches in diameter, on the cap of Bennet's electrometer, and with a piece of dry and warm window glaſs, ſcraped a piece of dry and warm beach, a few of the ſhavings being received on the tin plate, made the gold leaf ſtrike the ſides of the bottle with poſitive electricity. This was always the caſe whether the wood was hot or cold, but not always equally ſtrong.

with ſteel negative.

I thought a knife would be more convenient than glaſs to ſcrape with, and when I tried it, I found the ſhavings were negatively electrified, although they were taken from the ſame piece of wood which before gave poſitive.

Other woods.

This change in the reſult induced me to try different woods in different ways (ſometimes ſcraping and ſometimes cutting ſmall chips) but obtained very uncertain reſults, for ſometimes I obtained poſitive and ſometimes negative electricity, even when I cut the ſame piece of wood with the ſame knife.

An inſulated knife acquires the contrary electricity.

I next fixed the blade of a penknife into a glaſs tube, covered with ſealing wax, and ſet to work with this inſulated knife, and found that it was always electrified with the contrary electricity to that of the chips which were moſt frequently poſitive. But as they were ſometimes negative, I repeated the experiments very often to diſcover the cauſe, but with very little ſatisfaction.

A ſharp inſulated knife gave negative chips; a blunter poſitive.

However, after making ſeveral hundred trials, I found the keenneſs of the edge of the knife had ſome influence, for one day after chipping with the inſulated knife, and getting poſitive chips, I ſet the knife on a hone to make it cut better (which I had frequently done before) and when I began to chip the ſame piece of wood which but juſt before gave poſitive chips, I found the chips were negatively electrified ſeveral times, I then chipped the ſame piece of wood with a knife that had been very much uſed, without ſharping, and this gave poſitive chips as the other had done before it was ſharped; the knife that had been ſharped was tried again, but the chips were poſitive now, however when it was ſharped again it gave negative chips.

I now

I now thought I had found the cause of the contradictory results, but to be more certain about it, I began the following set of experiments. I sharpened a penknife to a very nice edge, and used the same pieces of wood that had been used in the former trials. In 24 trials with cherry-tree, the chips were always negative, and in four trials with elm, and in six with yew, the chips were always negative. I now drew the edge of the knife lightly over a piece of iron to dull it, expecting to get positive chips, but on trial the chips were still negative. Supposing the knife was not dull enough, I drew the edge over the iron again, and made the edge very bad, but the chips were still negative. The edge of the knife was next made rough by rubbing it on a grindstone, and this rough edge gave negative chips. The knife was next ground and set on the hone very carefully, and this sharp edge gave negative chips. Other contradictory experiments.

As nothing satisfactory was obtained from this set of experiments. I began to suspect that the degree of heat of the wood was to be considered (for the wood was cold in all the last experiments, and sometimes hot, and sometimes cold, but most frequently hot in the first) or that perhaps the heat of the wood, and the sharpness of the knife, were both to be taken into the consideration. I therefore set about the following set of experiments. Probability that the heat of the wood might influence the results.

I split the piece of cherry tree (that was used before) into two pieces, one of which was made thoroughly hot at the fire. This when chipped with the same knife that was used in the last experiments, without being sharpened, gave positive chips every time in six trials, and after this piece of wood had cooled till it was scarcely warm, gave positive chips every time in four trials. I then took the other piece which had not been near the fire for five or six hours, this gave negative chips every time in four trials. I now made this piece of wood quite hot, thoroughly, and chipped it again with the same knife, and in seven trials the chips were positive every time. These two pieces of wood was now laid by for three or four hours to get quite cool. In this state they gave negative chips every time in twelve trials. One of them when made thoroughly hot again, gave positive chips every time in six trials. The other piece was now made warm (but only externally so) in eight trials it gave positive chips four times, Detail of experiments with hot and cold wood; and different sharpness of the knife.

and

and negative chips four times; but after laying three or f hours to cool it gave negative chips every time in eight tri A third piece of cherry-tree that had not been near a fire f four or five days gave negative chips. I repeated theſe e periments with different knives that had not very ſharp edge and with beach as well as cherry-tree. And whenever the wood was made thoroughly hot at the fire, it always gave poſitive chips, not only when hot, but when it was ſo cooled as not to be ſenſibly warm; but when it had laid away from the fire three or four hours, the chips were always negative. Sometimes when the wood was but ſlightly warmed, it would be very difficult to get any ſigns of electricity, and at other times when the wood was made hot only externally (by putting it very near the fire for a ſhort time) the firſt few chips would be poſitive, and the ſucceeding ones negative. I had one inſtance where with the firſt chip the electrometer diverged near an inch, and with the ſecond it completely cloſed again. Having ſucceeded thus far, I thought I would try whether the reſults would be the ſame if I uſed a very ſharp knife, and accordingly ſharped two knives on a hone very carefully. And I uſed the ſame pieces of cherry-tree made thoroughly hot; in nine trials with one of the knives, the chips were negative every time and in five trials with the other knife, the chips were negative every time. I made a number of ſimilar trials with a piece of beach with always the ſame reſults, but the beach ſeemed to be too hard for that keenneſs which is neceſſary to produce negative chips, for after cutting one or two chips, the edge would be ſpoiled, and produce poſitive chips; but always when the knife was ſharped, the firſt one or two chips would be negative. Similar to this I found by ſubſequent trials to be the caſe with the pieces of cherry-tree, ten or twelve chips of this (which was very ſtraight and open grained) would ſpoil the edge.

General reſults. Shavings of dry wood by glaſs, are negative—hot wood by a moderate ſteel edge is poſitive, but negative if cold:—and a very ſharp edge gives negative whether hot or cold.

From theſe experiments it appears, that when very dry wood is ſcraped with a piece of window glaſs, the ſhavings are always poſitively electrified. And if chipped with a knife, the chips are poſitively electrified if the wood is hot, the edge of the knife not very ſharp, and negatively electrified if the wood is quite cold. But if the edge of the knife is very keen, the chips will be negatively electrified whether the wood is hot or cold.

2 The

The greatest number of trials was made with the insulated knife, which was always electrified contrarily to the chips; but the surface of the wood where the chips were cut from was very seldom electrified, and when it was it was always but weakly so, and of the same denomination as that of the weakest of the other two. I have repeatedly found that if a piece of dry and warm wood is suddenly split asunder, the two surfaces which were contiguous are electrified, one side positive and the other negative.

The knife always contrary.

Split wood has the two states.

X.

On the Composition of Emery. *By* SMITHSON TENNANT, *Esq. F. R. S.* (*Ph. Trans.* 1802.)

THE substance called emery, which, from its great hardness, has been long used in various manufactures, for grinding and polishing other bodies, has not, it appears, been hitherto correctly analyzed. In books of mineralogy, it is considered as an ore of iron; an opinion probably derived from its great specific gravity, as well as from the iron which it frequently contains. But, where this metal is most abundant, it could not be extracted from it with advantage, and ought rather to be regarded as an impurity, as it does not contribute to produce the peculiar hardness for which this substance is distinguished. In Mr. Kirwan's mineralogy, he mentions an examination of emery made by Mr. Wiegleb, from which he inferred that 100 parts consisted of 95,6 of silex, and 4,4 of iron. Mr. Kirwan, however, justly suspects the correctness of this account, and observes that he had no doubt but some other stone was imposed on Mr. Wiegleb for emery.

Emery has not yet been correctly analyzed.

When powder of emery is boiled in acids, it becomes of a lighter colour, from the loss of part of the iron; after which, it does not seem to undergo any further alteration. As acids produce so little effect on it, I exposed it to a pretty strong red heat, with carbonate of soda, in a crucible of platina. On adding water to the mass contained in the crucible, the greater part of the emery was found in powder; having only become of a light colour, from the extraction of part of the iron.

Emery powder gives part of its iron to acids.

Soda by fusion dissolved some argil and leaves the emery.

iron. Though this process was twice repeated with the remaining powder, and in a stronger heat, a great porportion of it remained undissolved.

The alkaline solution, after a red calx of iron had subsided from it, was saturated with acid; and gave a precipitate of a white earth, which I found to be almost purely argillaceous.

This result is similar to Klaproth's with diamond spar.

The result of these experiments, was so similar to those of Mr. Klaproth on diamond spar, as to render it very probable that emery was in reality the same substance, though usually mixed with a larger proportion of iron; and the subsequent experiments appear to confirm this opinion.

Emery pulverized and cleared of magnetic particles.

In order to obtain a quantity of emery as free from iron as I could, I reduced to a coarse powder, a piece which consisted of different strata, some of which were of much lighter colour than others; and afterwards separated, by a magnet, the particles which were attracted by it. The part which was not attracted by the magnet, I observed to have the usual degree of hardness (by the scratches which might be made with it on flint.) I then reduced it to a finer powder, in an agate mortar; and, as this was principally done by pressure, and not by grinding, hardly any sensible addition was made to its weight. In the same manner, I found that diamond spar might be powdered to the same degree of fineness, without any material increase of weight from the mortar.

20 of the clear powder fused with 120 soda,

Of the emery powder thus prepared, 20 grains were taken, and heated in the manner before described, with 120 grains of soda, which had been previously deprived of carbonic acid, and boiled to dryness in a silver pan. By nearly the same process as that used by Mr. Klaproth, I obtained about 16,0 grains of argillaceous earth, ,6 of siliceous earth, ,8 or ,9 of iron, and ,6 of a grain remained undissolved. These numbers, reduced to parts of a hundred, are therefore,

and by the same process as Klaproth's the component parts of emery

Argillaceous earth - - - - -	80
Silex - - - - - -	3
Iron - - - - - -	4
Undissolved - - - - - -	3
	90.

Mr.

Mr. Klaproth obtained from the Chinese corundum, after separating from it the particles which were attracted by the magnet,

proved nearly the same as those of Chin corundum.

Argillaceous earth - - -	84
Silex - - - -	6,5
Iron - - - - -	7,5
	98.

As this analysis was no doubt conducted with greater care than mine, the loss of weight was less; but the proportion of the ingredients is sufficiently near to show that the substances are essentially the same.

From 25 grains of emery which appeared the most impregnated with iron, and yet retained its usual hardness, I obtained, argillaceous earth 12,5, silex 2, iron 8, and one grain was not dissolved; or, per cent.

Other emery containing one third part iron

Argillaceous earth - - -	50
Silex - - - - -	8
Iron - - - - -	32
Undissolved - - - -	4
	94.

As such emery can easily be had of uniform quality in large pieces, I procured the powder employed in this experiment, by rubbing two pieces against each other.

From 25 grains of emery, similar in appearance to the preceding, but which had been digested with marine acid previous to the action of the alkali, I had,

Another analysis.

		per cent.
Of argillaceous earth - - -	16,4	65,6
Siliceous earth - - -	,8	3,2
Iron - - - - -	2,	8,
Not dissolved - - - -	4,5	18,0
	23,7	94,8.

The hardness of emery, as far as I could judge by its cutting rock crystal and flint, appeared to be equal to that of diamond spar. The latter could not be scratched by the former; but, as emery has not a surface sufficiently polished to render a mark visible, the converse of this could not be tried.

Emery appears as hard as diamond spar.

All

Emery comes chiefly from Naxos.

All the emery which is uſed in England, is ſaid to be brought from the Iſlands of the Archipelago, and principally from Naxos. In thoſe places, it is probably very abundant; as the price of it in London, which I was told was 8 or 10 ſhillings the hundred weight, appears little more than ſufficient for the charges of carriage. Though I ſaw a very large quantity in one place, (more than a thouſand hundred weight,) I could not find any pieces of a cryſtallized form; poſſibly the great proportion of iron uſually mixed with it, may prevent its cryſtallization. The whole conſiſted of angular blocks incruſted with iron ore, ſometimes of an octaedral form, with pyrites, and very often with mica. The latter frequently penetrates the whole ſubſtance of the maſs, giving it, when broken, a ſilvery appearance, if ſeen in the direction in which the flat ſurfaces preſent themſelves to the eye. As theſe ſubſtances have no chemical relation to the emery itſelf, it is remarkable that they ſhould alſo accompany the diamond ſpar from China; for Mr. Klaproth obſerves, "that its lateral "facets are moſtly coated with a firmly-adhering cruſt of "micaceous ſcales, of a ſilvery luſtre:" he alſo mentions, beſides felſpar, pyrites, and grains of magnetic iron ore.

It is cheap; not cryſtallized; is accompanied by the ſame ſubſtances as Chineſe diamond ſpar.

XI.

Experiments and Obſervations on the Power of Fluids to conduct Heat; with Reference to Count Rumford's Seventh Eſſay on the ſame Subject. By JOHN DALTON *.

The properties of heat are continually under our notice.

THE nature and properties of fire or heat are ſubjects which preſent themſelves to our conſideration in almoſt every department of phyſics: it is no wonder therefore that new experiments, which point out and define the modes of operation of fire, before unobſerved, or at leaſt too much overlooked, ſhould attract the attention of philoſophers.—Theſe obſervations were ſuggeſted upon reading Count Rumford's very ingenious experiments, in his eſſay above-mentioned, which exhibit a fact in a more ſtriking point of view than it has ap-

Count Rumford's experiments on the tranſmiſſion of heat by circulation through fluids.

* Mancheſter Memoirs, vol. v. 373.

peared

peared before—namely, *that the quickneſs of the circulation and diffuſion of heat in fluids, is occaſioned principally by the internal motion ariſing from a change of ſpecific gravity affected by the heat.*—But the concluſion he has drawn from them—*that fluids are perfect non-conductors of heat,* in the way in which ſolids conduct it, appears to me totally unwarranted from the experiments, and erroneous in itſelf. And as it may be an error of practical conſequence, if adopted, the expoſition of it ſeemed deſirable—which is the object of the following remarks and experiments.

His inference, that fluids are non-conductors erroneous.

My firſt attempt was to aſcertain the preciſe degree of cold at which water ceaſes to be further condenſed—and likewiſe how much it expands in cooling below that degree to the temperature of freezing, or 32°. For this purpoſe I took a thermometer tube, ſuch as would have given a ſcale of 10 inches with mercury from 32° to 212°, and filled it with pure water. I then graduated it by an accurate mercurial thermometer, putting them together in a baſon filled with water of various degrees of heat, and ſtirring it occaſionally: as it is well known, that water does not expand in proportion to its heat, it does not therefore afford a thermometric ſcale of equal parts, like quickſilver.

Experiments.

A thermometer made with water.

From repeated trials agreeing in the reſult, I find, that the water thermometer is at the loweſt point of the ſcale it is capable of, that is, water is of the greateſt denſity at $42\frac{1}{2}°$ of the mercurial thermometer. From 41° to 44° incluſively the variation is ſo ſmall as to be juſt perceptible on the ſcale; but above or below thoſe degrees, the expanſion has an increaſing ratio, and at 32° it amounts to $\frac{1}{8}$th of an inch, or about $\frac{1}{160}$th part of the whole expanſion from $42°\frac{1}{2}$ to 212° or boiling heat.—During the inveſtigation of this ſubject, my attention was arreſted by the circumſtance, that the expanſion of water was the ſame for any number of degrees from the point of greateſt condenſation, no matter whether above or below it: thus I found that 32°, which are $10°\frac{1}{2}$ below the point of greateſt denſity, agreed exactly with 53°, which are $10°\frac{1}{2}$ above the ſaid point; and ſo did all the intermediate degrees on both ſides. Conſequently when the water thermometer ſtood at 53°, it was impoſſible to ſay, without a knowledge of other circumſtances, whether its temperature was really 53°, or 32°. Recollecting ſome experiments of Dr.

Greateſt contraction at $42°\frac{1}{2}$.

Law of expanſion.

It is the ſame on each ſide above and below $42°\frac{1}{2}$.

Dr. Blagden in the Philofophical Tranfactions, from which it appears that water was cooled down to 21° or 22° without freezing, I was curious to fee how far this law of expanfion would continue below the freezing point, previoufly to the congelation of the water, and therefore ventured to put the water thermometer into a mixture of fnow and falt, about 25° below the freezing point, expecting the bulb to be burft when the fudden congelation took place. After taking it out of a mixture of fnow and water, where it ftood at 32° (that is 53° per fcale) I immerfed it into the cold mixture, when it rofe, at firft flowly, but increafing in velocity, it paffed 60°, 70°, and was going up towards 80°, when I took it out to fee if there was any ice in the bulb, but it remained perfectly tranfparent: I immerfed it again, and raifed it 75° per fcale, when in an inftant it darted up to 128°, and that moment taking it out, the bulb appeared white and opake, the water within being frozen: fortunately it was not burft; and the liquid which was raifed thus to the top of the fcale was not thrown out, though the tube was unfealed. Upon applying the hand, the ice was melted, and the liquid refumed its ftation. This experiment was repeated and varied, at the expence of feveral thermometer bulbs, and it appeared that water may be cooled down in fuch circumftances, not only to 21°, but 5° or 6°, without freezing, and that the law of expanfion abovementioned obtains in every part of the fcale from 42°½ to 10° or below; fo that the denfity of water at 10° is equal to the denfity at 75°. But as the difcovery of this curious, and I believe hitherto unnoticed property, has little to do with the object before us, I fhall fay no more of it at prefent.

and the water may be cooled down as low as 5° or 6° without freezing, &c. &c.

(To be continued in our next.)

SCIENTIFIC

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Lectureship on Subjects of Natural and Experimental Philosophy, at Newcastle upon Tyne.

AMONG our provincial societies for promoting the cultivation of literature and philosophical pursuits, the society at Newcastle upon Tyne, has for a number of years possessed an eminent station for the ability of its members, and the unremitting assiduity and intelligence which they have exerted to discover and carry into effect those objects which bodies of men are best capable of promoting. We all know that it is easy for a body of men to assemble together; to dignify themselves by a name; to hold forth striking pretensions to the uninstructed multitude; and to make a sort of commercial traffic of that scientific celebrity which the voices of men can confer upon each other, for a time at least, while the great public may ask in vain what they have done to deserve it. But when a body of true friends to their country and to mankind meet together, to consider in what way they can best employ their talents for the good of society and the promotion of the arts of civilized life, within that district which more immediately falls under the limit of their notice, and the extent of their power and influence;—when their well-earned celebrity is a secondary object, in comparison with the internal satisfaction afforded by a proper employ of talents and virtue—such an association becomes a public blessing, and its good consequences extend to the remotest periods of after times. The Literary and Philosophical Society of Newcastle, has already made inquiries into the state of the arts, of agriculture, and of the mineral products of their vicinity. It has assisted the diffusion of knowledge by the establishent of a well regulated and easily accessible library; and it has lately proceeded to constitute a lectureship on the subjects of natural and experimental philosophy, the requisite steps for which are now in progress with great spirit and activity.

Establishment of a philosophic lecture at Newcastle upon Tyne.

Active exertions of the society at Newcastle.

On the fourth of May last, a paper by Mr. Thomas Bigge, on the expediency of establishing a lectureship at Newcastle upon Tyne, upon the subjects of natural and experimental philosophy, was read: In this paper, we find a clear, elegant and

History of the proposed lecture.

Mr. Bigge's proposal

and animated ftatement of its objects; confidered as well with regard to their great influence on fociety, as to thofe local circumftances towards which the intention of the author was more particularly directed. In this paper, no lefs ftriking for the value of its contents, than for the eftimable motives of its author, we find facts which ftrongly fupport what has been faid at the commencement of this article concerning the general proceedings of this refpectable fociety. In confequence of this fuggeftion, the fubject was again refumed, and an addrefs to the public was circulated in the month of June, requefting their co-operation and affiftance, exhibiting fome part of the outline, and announcing that the appointment of Lecturer has been made to the Rev. Wm. Turner, a gentleman long and well known to the fociety for the abilities with which he had exercifed the office of fenior fecretary. Since that time, confiderable progrefs has been made in the fubfcriptions; and the lecturer delivered a general introductory difcourfe on Tuefday, November the 16th laft, upon the objects, the advantages, and the intended plan of the lectures. I am forry that the limits of a fhort ntoice forbid the attempt to give any analyfis of this excellent difcourfe, which affords ample evidence of the comprehenfive views of the author, with regard to the organization and duties of fociety in general, as well as the fciences he has undertaken to teach. I have thought it might be advantageous in fome degree to the views of this public body to give the prefent account, fhort and imperfect as it muft neceffarily be, and ftill more to the world at large, if the exertions in favour of the fciences in one part of the kingdom, fhould, as is moft probable, be followed by fimilar proceedings in others.

adopted, and an addrefs to the public circulated.

On a new Kind of Mortar called Plaifter Cement.

Account of a water cement.

Among the ftones on the fea-beach, near Boulogne, a particular kind is collected, which when calcined and pounded like plaifter, forms a very hard cement with water. This fubftance has been ufed for œconomical purpofes, and was found to poffefs the valuable quality of refifting water; under which fluid, it hardens very ftrongly, and much more than in the

the air. Many conſtructions made with this cement afford the moſt complete proof of its ſolidity and tenacity. The detail of theſe experiments are given in a report preſented by *Le Suge* to the ſociety of Agriculture, Commerce and Arts, at Boulogne. Theſe muſt no doubt be of much local importance; but the analyſis of Guyton, as it tends to elucidate the compoſition of cements, is of more extended conſequence.

The ſpecific gravity was between 2,04 to 2,19 and 10 grammes, or about $\frac{1}{3}$ of an ounce produced in centegrammes.

Lime	403
Carbonic acid	330
Clay	187
Oxide of iron	70
Alumine	5
	995

The 187 centegrammes of clay afforded,

Silica	99
Alumine	39
Oxide of iron	43
	181

The ſtones are therefore compoſed of

Lime	403
Carbonic acid	330
Oxide of iron	113
Silica	99
Alumine	44
Loſs	11
	1000

Citizen Guyton ſhewed a veſſel to the ſociety d'Encouragement at Paris, which was very cloſe and firm in its texture, whence the abridger in the bulletin des Sciences, from which I take this article, expreſſes his opinion that it would prove very uſeful in the fabrication of various articles of pottery.

Not

Note concerning Two Brothers of a Race of Men resembling Porcupines.

Men having ſcales and ſpines like porcupines.

Many philoſophers have already ſpoken concerning this race, the reality of which has been eſtabliſhed in a family well known by the name of Lambert. Two brothers of this family, all the males of which, have their bodies covered with ſpines or ſcales, are at preſent in Paris. One is 24 and the other 14 years of age. The body of the eldeſt is entirely covered, except the head and the inſide of the hands and feet: the youngeſt is naked in ſome places, particularly about the breaſt; but the brown ſpots on thoſe parts ſufficiently indicate that when he advances in age, he will become as rough as his brother. The ſpines on the back of the hand are very large, and may be compared for their diameter to the quills of the porcupine; but thoſe upon the breaſt have a greater reſemblance to ſcales: they are ſmall long plates, very numerous, and near together, being vertically implanted in the ſkin.

This thickening of the epidermis and hair, proceeds from a diſeaſe tranſmitted from generation to generation, but only from male to male. Five generations have been already afflicted with it.

Bull. de la Soc. Philomath.

Mawe's Mineralogy of Derbyſhire.

The Mineralogy of Derbyſhire, with a Deſcription of the moſt intereſting Mines in the North of England, in Scotland, and in Wales; and an Analyſis of Mr. Williams's Work, entitled, "The Mineral Kingdom." Subjoined is a Gloſſary of the Terms and Phraſes uſed by Miners in Derbyſhire. By JOHN MAWE, *London. Phillips, George Yard,* 1802, *octavo,* 211 *Pages, with* 4 *Engravings.*

The author of this uſeful book was employed by a Spaniſh gentleman to make ſurveys of our principal mines in Derbyſhire, to collect their various productions, and more particularly ſpecimens from each ſtratum, deſcribing their thickneſs, ſituation, and poſition; in order to ſhew an exact repreſentation of the mines for the royal cabinet at Madrid. To the

 obſervations

obſervations thus made, he has added accounts of ſome mines in Scotland; a Tour from Glaſgow to Staffa; the ſalt mines at Northwich; the Parys Mine; Obſervations in Wales, and an Account of Mr. Williams's book, in 2 vols. called the Mineral Kingdom. This account ſeems too conciſe to be of great utility, and as a critique would have anſwered its purpoſe as well, if a few of the epithets had been more civil.

An Enquiry into the Cauſes of the Errors and Irregularities which take place in aſcertaining the Strength of Spirituous Liquors, by the Hydrometer, with a Demonſtration of the Practicability of ſimplifying and rendering this Inſtrument accurate. By WILLIAM SPEER, *Superviſor and Aſſayer of Spirits in the Port of Dublin. Octavo,* 48 *Pages. London,* 1802. **Speer on the hydrometer.**

On the preſent occaſion, when the government of the country appears diſpoſed to ſettle this point of experimental philoſophy, upon which reſults of conſiderable importance to the revenue and to private property depend, but which have certainly been hitherto conducted in a ſlovenly and inaccurate manner, it muſt be very acceptable to all parties to ſee the ſubject clearly treated in a ſmall pamphlet. After a ſhort Introduction, the author gives an account of the origin and riſe of the hydrometer, the cauſes of its irregularities, the different modes of charging overproof; the principal defects in Clarke's hydrometer; the imperfection of the weights in that inſtrument; the general principles of floating inſtruments; the ſtandard of pure ſpirit; Gilpin's tables; modes of ſimplifying the hydrometer; deſcription of his own improved hydrometer: with anſwers to objections that probably might be made to it; and he concludes with a chapter on the neceſſity of a ſtandard for proof, and other practical objects of conſequence in this buſineſs.

M. Speer's own hydrometer conſiſts of a ball and ſtem, with counterpoiſe beneath as uſual, and the upper or graduated ſtem is made of an octagonal form. Upon each of the eight faces is engraved a ſcale of per-centages; by inſpection of which, the quality of the ſpirit is ſeen. But as the inſtrument will ſink to different depths in the ſame ſpirit, according to its

Deſcription of Speer's hydrometer.—Stem octagonal; each ſide having the *per-centages* for different temperature.

Speer on the Hydrometer.

its temperature, the ſcale upon each of the faces is adapted to a determinate temperature, namely 35°, 40°, 45°, &c. till the laſt, which is for 70°. The temperature of the ſpirit being therefore known, the reſult muſt be read, upon that face, at the top of the which the known temperature is engraved; and to prevent any miſtake, there is a ſmall index to be put on the ſtem to direct the eye of the obſerver to the proper face. And moreover, as the temperature is ſhewn only to every five degrees, there is another index of a different colour, which performs the office of a weight, and ſhews the intermediate temperature. This may alſo be effected by warming the ſpirits, by holding the glaſs in the hand till its temperature agrees with that marked on one of the faces; or, as I underſtand from the ſpecification of the author's patent, the preciſion of a ſingle degree, if required, may alſo be obtained, by four ſmall pins to be inſerted, one for each intermediate degree in holes, in the counterpoiſe below, where in fact they operate as weights of adjuſtment.

Mr. Harriotts Hydraulic Engine for raising & lowering weights & for other purposes

Fig. 1.

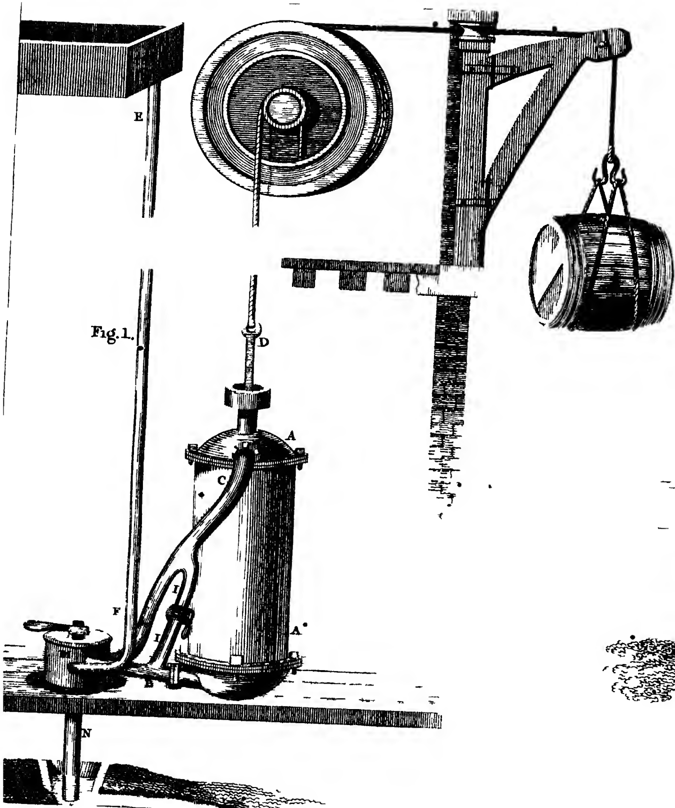
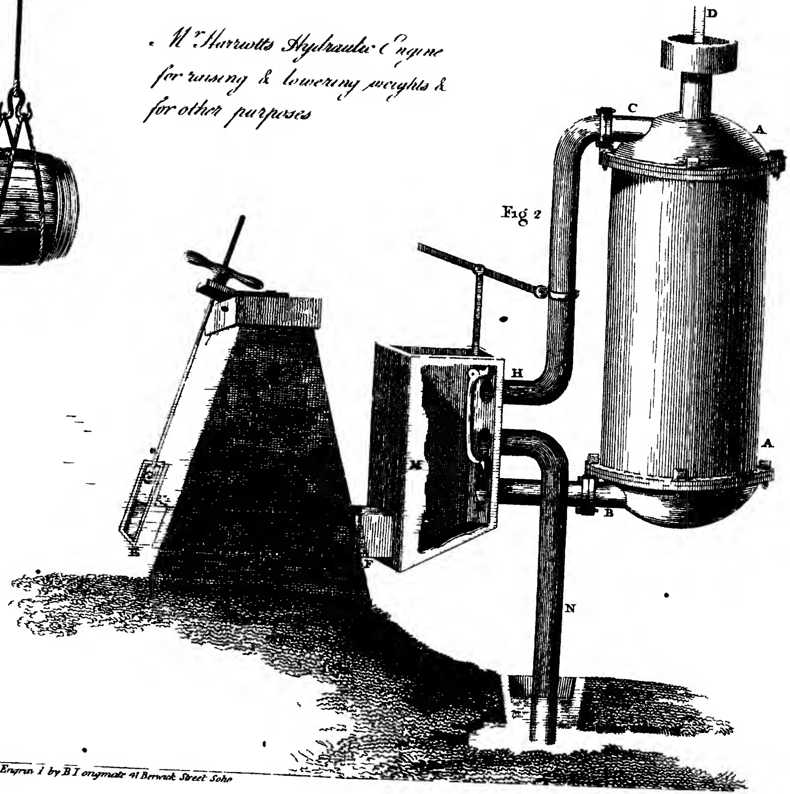
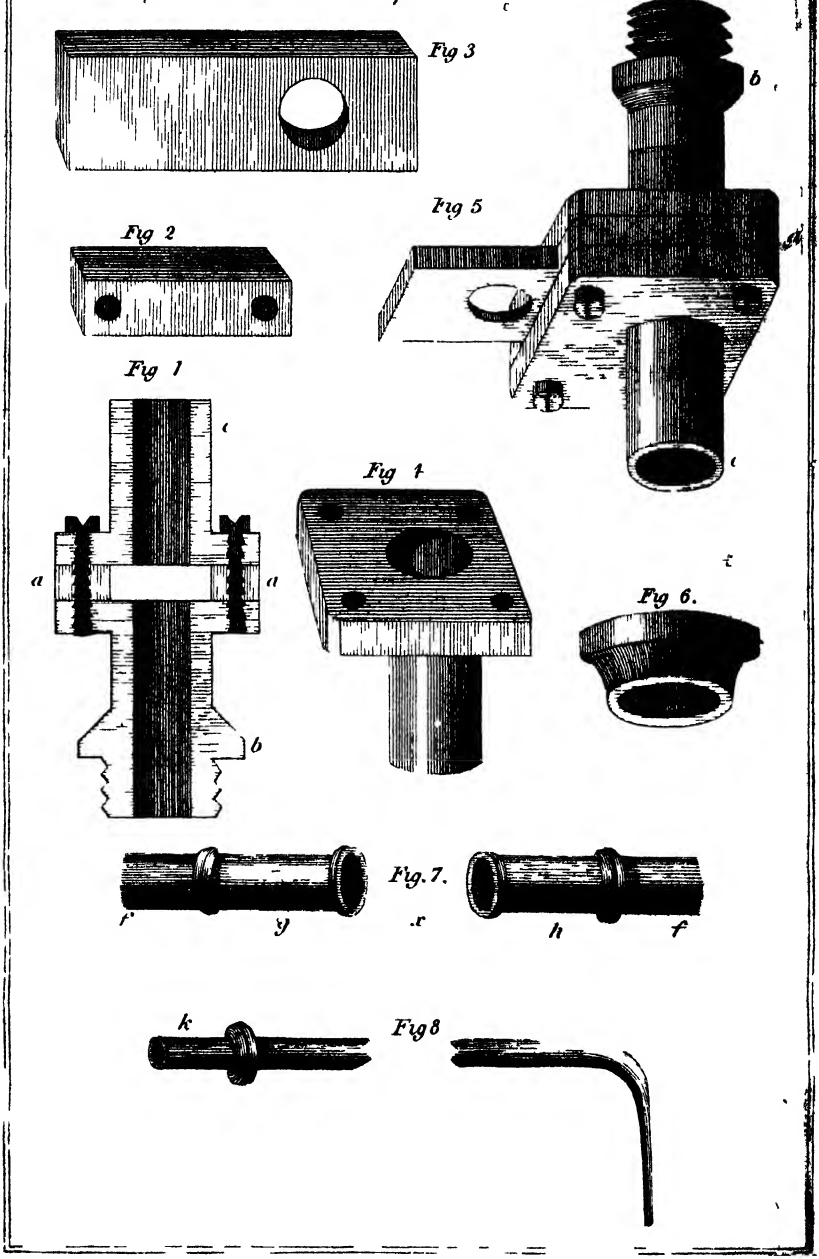


Fig 2

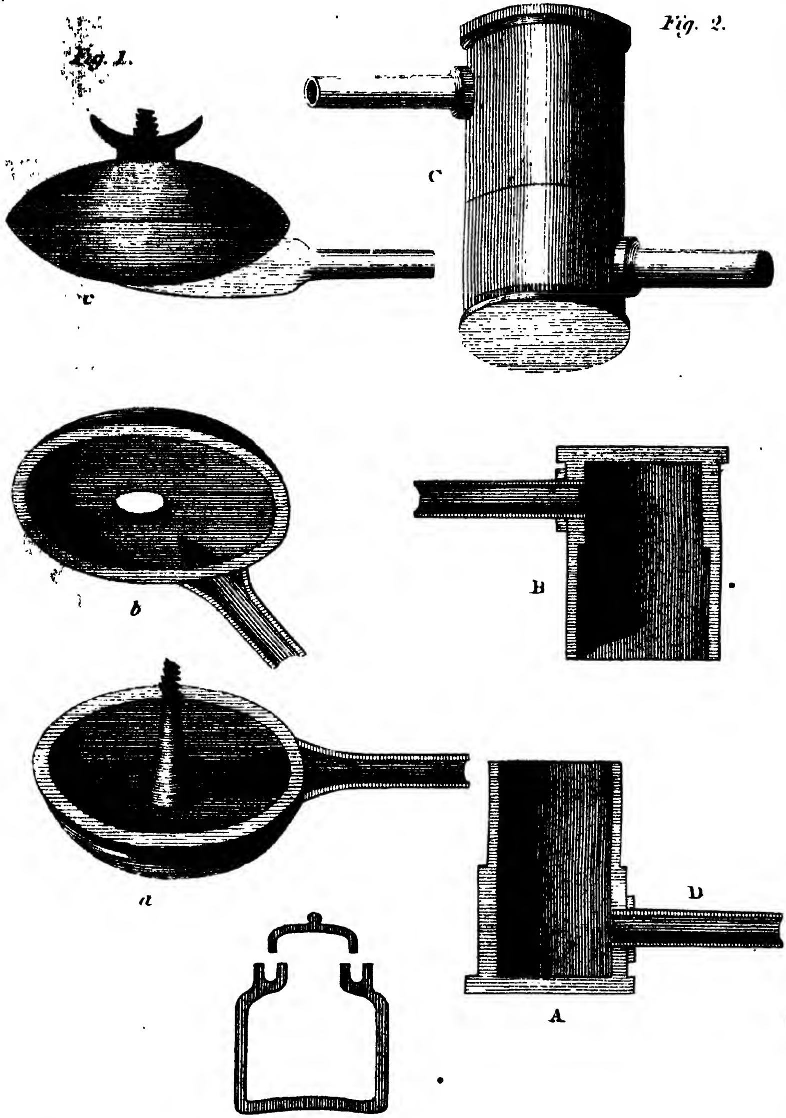


Drawn & Engrav.d by B Longmate 41 Berwick Street Soho

Sliding Stop cock for pneumatic Experiments



Metallic Joints for pneumatic Apparatus



Mutlow sc Russell Cor

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

FEBRUARY, 1803

ARTICLE I.

Anſwer to the Obſervations of Mr. WILLIAM CRUICKSHANK *upon the Doctrine of Phlogiſton* *. *In a Letter from the Rev.* JOSEPH PRIESTLEY, *L. L. D. F. R. S. &c.*

To Mr. NICHOLSON.

Northumberland, America, Nov. 5, 1802.

SIR,

YESTERDAY only I received the fifth Number of your valuable Journal, viz. for May, which contains Mr. Cruickſhank's reply to my defence of the doctrine of phlogiſton in anſwer to him; and it gives me ſincere pleaſure to obſerve, that he ſhews ſo much zeal in the defence of his hypotheſis, as by this means the truth will appear the ſooner. I only lament, that my great diſtance will neceſſarily make the controverſy of much longer continuance than it would otherwiſe be. Introductory paragraph.

Mr. Cruickſhank cenſures me for miſquoting his words, and miſtaking his meaning. If I have done ſo, I can aſſure him that it was without deſign; nor do I perceive that what he The quotation diſcuſſed by Mr. Cruickſhank ſtated to be of no conſequence to the argument.

* Philoſophical Journal, II. 48.

alledges of this kind is of any confequence to the main argument. On the other hand, he fays, p. 42, with an air of triumph, but before the victory, that "having additional difficulties to ftruggle with, I have been under the neceffity of adopting new, and fometimes contradictory, opinions, in my explanations and defences."

The difcuffion attended with no difficulty.

Now I really am not fenfible of any difficulty whatever in this difcuffion; and in advancing this he feems not to have apprehended my fentiments with refpect to the calces of metals. If he was acquainted with my publications, he would have found that there was nothing either new, or contradictory, in what I have now advanced on this fubject, or that of the compofition of charcoal; though that I have fometimes changed my opinions on philofophical as well as other fubjects, I readily acknowledge, and without any feeling of fhame.

Finery cinder contains water only. For (upon adding phlogifton) water only is feparated.

In my opinion, and that of long ftanding, the black calx of iron, commonly called *finery cinder*, contains no oxigen, but only water; and though the calx of zinc, and the yellow calx of lead, called *mafficot*, do contain fome, it is fo little that I cannot detect it. Mr. Cruickfhank fays, p. 42, that "if water be the only fubftance contained in oxides, heat alone ought at leaft to revive fome of them, and that in this cafe nothing but water would be feparated." Now what I maintain is, that when finery cinder is revived, (which, however, is not done without the introduction of phlogifton) nothing but water is feparated from it.

The prefent letter is confined to that object.

Oxigen is undoubtedly contained in the red calx of lead, called *minium*, and in that of mercury; but I fay that even thefe cannot be revived without the introduction of phlogifton. This fubject, however, I wave for the prefent, wifhing to difpatch that of the air from finery cinder and charcoal before we proceed to any other; and I wifh Mr. C. to attend to the following obfervations, which I fhall now ftate more diftinctly than I have done before, that it may be the eafier for our readers to judge between us.

Mr. C's theory demands that oxigen fhould quit iron to form fixed air with charcoal, and then return to the iron in order

1. Mr. Cruickfhank's hypothefis requires that, in the procefs of heating finery cinder and charcoal, the oxigen in the finery cinder fhould quit that fubftance, and unite with carbon in the charcoal, in order to form fixed air. Since, however, this fixed air is to be decompofed by iron, the oxigen which it

4 has

has got from the finery cinder muſt be ſeparated from it, and enter into the ſame calx again. But while the heat continues the ſame, I deem theſe contrary effects to be impoſſible. If the degree of heat that is applied expel oxigen from the calx, it will certainly prevent its return. Conſequently, if fixed air be formed, it cannot be decompoſed in theſe circumſtances.

to leave oxide of carbon; which cannot happen at the ſame heat.

2. If it was poſſible for oxigen to be ſeparated from finery cinder without any thing entering into it, which I think cannot be done, it could not, according to the new theory, form any thing by its union with carbon beſides *fixed air*; this being ſaid to be carbon diſſolved in oxigen gas, but nothing inflammable could be produced by their union. Of this Mr. Cruickſhank is ſenſible, and therefore he was under the neceſſity of ſuppoſing that, after the oxigen had quitted the finery cinder, it muſt enter into it again. But if this were poſſible, nothing would remain of the fixed air but *carbon*, as before that union; and this is a ſolid ſubſtance, incapable, without the aid of oxigen, of aſſuming the form of air. Whence, then, comes the inflammable air in this proceſs, which ſo nearly reſembles that from charcoal and water, that they muſt have the ſame origin; and with reſpect to *this*, Mr. Lavoiſier decides, that whatever is inflammable in it muſt come from water.

Carbon being conſidered as forming only one compound with oxigen (namely fixed air) it is concluded that no inflammable air could be had in reducing finery cinder; unleſs by means of water.

3. Admitting all that Mr. Cruickſhank alledges concerning the difference in ſpecific gravity, and other circumſtances, between the air from finery cinder and charcoal, and that from water and charcoal, it is not ſo great as the difference between the latter and the light inflammable air from the metals with acids or water. Different as they may be in other reſpects, they are all inflammable, and have the common property of uniting with oxigen in a certain degree of heat; in conſequence of which they are the reverſe of *oxides*, and muſt be claſſed with *combuſtible* ſubſtances, equally with ſulphur and phoſphorus.

The inflammable gas, though different from that obtained by charcoal and water, is not an oxide but a combuſtible.

4. If the oxigen, after quitting the finery cinder, entered into it again, it would make it finery cinder as at the firſt, or at leaſt in ſome degree; whereas the calx is completely revived in this proceſs, the iron ſo revived being as ſoluble in acids as any iron whatever.

The oxigen does not return to the finery cinder; for the iron is reduced and continues ſo.

5. If the iron ſhould be completely revived by the oxigen wholly leaving it, I ſtill maintain that it could not, by any degree

Iron cannot decompoſe fixed air. Mr. C's experiment leſs

decifive than that of the author.

degree of heat decompofe fixed air. For my experiment with a *burning lens*, in which it could not be done, is far more unexceptionable than Mr. Cruickfhank's with *bladders and a gun barrel*. His objection to my procefs has no weight. It was made with a few ounce meafures of the air over mercury, with a lens fixteen inches in diameter, and continued feveral hours, generally from ten o'clock to one; fo that no particle of the air could efcape being expofed to a far greater degree of heat than could be communicated through a gun barrel. His experiment I have frequently made both in England and here, but could never be fatisfied with the refult. The fcale upon the iron, I have no doubt, came from moifture in the air, or from the bladders. Indeed, I cannot think that any perfon converfant as I have been with both thefe modes of operating, can hefitate in deciding that the preference muft be given to mine.

Charcoal inferred to contain oxigen from Mr. Tenant's experiments.

6. Mr. Cruickfhank feems to think that charcoal cannot contain any oxigen; but Mr. Tenant's fine experiment decifively proves that it does. For where are we to look for the oxigen (which we all acknowledge to be a component part of fixed air) which is feparated from the marble, but in the charcoal which is produced, and in that it makes part of a folid fubftance, and does not take the form of air.

If oxigen exifted in finery cinder it ought to produce the phenomena of combuftion in charcoal.

7. Since oxigen and all combuftible fubftances unite, and explode, in a certain degree of heat, the oxigen that is expelled from the finery cinder uniting with carbon from the charcoal when red hot muft enable it to *burn*; and therefore in thefe circumftances there ought to be an explofion, or at leaft a gradual combuftion of them in the courfe of the procefs, as there is when oxigen is put to the fame fubftance, and heated with it afterwards.

Invitation to Mr. Cruickfhank to enter into a full difcuffion of the new theory.

It is now near twenty years fince this new theory was advanced, and from that time to the prefent I have not ceafed to exprefs my opinion of its fallacy, and to give my reafons for that opinion; but I have not till very lately been able to draw any degree of attention to the fubject. Now, however, I am happy to have fucceeded in this; and as I find that the chemifts in France, the great patrons of the fyftem, look to Mr. Cruickfhank as the ableft defender of it, I earneftly wifh that he would undertake the difcuffion of every article of my objections to it. What he has animadverted upon is only *one*

2 out

out of *eleven* articles in my *Tract on Phlogiston*; and besides this, he should consider what I have advanced on the same subject in other publications, especially my experiments on the generation of air from water, both by evaporation and by freezing, and those on the pile of Volta, and also several articles in the Transactions of the Philosophical Society at Philadelphia.

As to the manner in which a controversy of this kind is conducted, whether it be expressive of *respect*, or *contempt*, it will have little weight with a judicious reader. Preferring however a plain and calm discussion,

I am,

Dear SIR,

Your's sincerely,

J. PRIESTLEY.

II.

The Construction of an Apparatus for conducting Sound and holding Conversations at a Distance. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

SIR,

SOME of the most important inventions were at first made use of, either as playthings for children, or in exhibitions to amuse the populace.

Several very important inventions were at first considered as trifling.

Printing.

Printing, which has contributed more to the improvement of the human mind than any other art, was invented for amusement, and the instruction of children *. But the true value of an invention is not immediately seen, it has to go through a long series of improvements, before it arrives at a degree of perfection that is important to society.

Effects of the loadstone.

The loadstone was long known to jugglers, and used by them in their exhibitions, before it was applied to the important purpose of navigation: and how far the improvements in

* History of Holland by Adrian Young.

mechanics,

mechanics, optics, and acouftics, which are now exhibiting for amufement, may hereafter become ufeful in the common affairs of men, is a matter on which we are not at prefent able to judge.

Phantafms by the camera obfcura behind a fcreen.

The daggers and death-heads which are made to appear in the air, by means of a concave mirror; and the ghofts and goblins, that are conjured up by means of the magic lantern of Philipfthal, not only ferve as amufement, but they may contribute to check the growth of fuperftition, by fhewing in an agreeable manner how eafy it is to impofe upon the fenfes.

Speaking machines.

Another way of deceiving the fenfes, is by fpeaking machines. Thefe are of very ancient date, and have been fo much improved by the moderns, as to attract much attention.

One by Thomas Irfon in the 17th century.

No longer ago than the reign of Charles II. one Thomas Irfon excited much wonder in the king and his whole court, by a machine of this kind *. It is now well known that the found was conveyed to the mouth of the ftatue by means of tubes artfully concealed; but the principle on which the fpeaking machines are now conftructed, appears to be at this time very little underftood.

Sound conducted through timber.

It was known in the days of Pliny, that if a long beam of timber received a flight ftroke at one end, the found was diftinctly heard by a perfon whofe ear was applied to the other end, though it could not be heard at the fame diftance through the air.

This property applied to a fpeaking apparatus.

From the following experiments which I made on this property of wood, it appears that acouftic inftruments may be conftructed for converfing at a diftance, without the affiftance of tubes to convey the found.

EXPERIMENT 1.

A deal rod 16 feet long was by contact made the medium between two trumpets.

I took a deal rod 16 feet long, and about an inch fquare, and after having fixed one end of it into the fmall end of a fpeaking trumpet, I laid it upon two fupporters or props in an horizontal pofition. One of the props was placed under the trumpet, about three inches from its wide end, and the other prop was placed near the other end of the rod. Another fpeaking trumpet was then laid acrofs the rod, about three inches from the end. The wide end of this trumpet

* Beckmann's Hiftory of Inventions, III. 334.

rested upon the rod, but the other end was suspended by a ribband, from an object which need not be described.

After the apparatus had been prepared as above mentioned, I introduced my watch into the end trumpet, and on applying my ear to the cross trumpet, I heard the beats much louder than if the watch had been at the distance of a few inches only. The sound appeared to come out of the cross trumpet, although the watch was at the distance of seventeen feet and a half: and when the watch was laid into the cross trumpet, it was heard equally well at the end trumpet.

Sound was conveyed very distinctly through this apparatus.

It may not be improper to mention, that the sound was increased by introducing a piece of metal, between the cross trumpet and the rod upon which it rested; but the manner of producing the best effect, I found by trial.

EXPERIMENT 2.

I placed another prop under the middle of the rod, and laid several books upon it in different places; but the rod conveyed the sound just as well as before.

Bodies in contact with the rod did not prevent the effect.

EXPERIMENT 3.

My assistant in these experiments being seated at the end trumpet, and myself at the other, a conversation took place through this apparatus; but in whispers too low to be heard through the air at that distance. The conversation afforded us much entertainment; for when the ear was placed in a certain position, the words were heard as if they had been spoken by an invisible being within the trumpet. And what rendered the deception still more pleasing, the words were more distinct, softer, and more musical, than if they had been spoken through the air.

Conversation held by sound passing through the rod both ways in a whisper.

SPEAKING MACHINES.

What I have to say on this subject, must be so far anticipated by these experiments, as to render any particular description of mine unnecessary. It may not, however, be improper to mention my conjectures, respecting some useful purposes to which this acoustic instrument may be applied. If a communication founded on these principles, were made between a shop and the dining room, or warehouse, it might contribute to the dispatch of business: and instruments made

Useful purposes to which these machines may be applied.

on

on the ſame principles might, perhaps, be found convenient in private houſes, if introduced between the parlour and ſome other room appropriated to the uſe of domeſtics. Directions might then be given to a ſervant without his entering the room, and in whiſpers too low to diſturb the company.

I am,

SIR,

your's reſpectfully,

EZEKIEL WALKER.

Lynn Regis, Jan. 19, 1803.

ERRATA.

Vol. IV. p. 40. in the firſt col. of the table, *for* 1, 2, 2, 4, *read* 1, 2, 3, 4.
P. 43. line 12, *for* long *read* large.
P. 43. line 19, *for*, In this manner, *read* Thus.

III.

Obſervations in Reply to Mr. GOUGH's *Letter on the Grave Harmonics* *. *In a Letter from* THOMAS YOUNG, *M. D. F. R. S. &c.*

To Mr. NICHOLSON.

SIR,

It is the opinion of Lagrange that the grave harmonics are not the reſults of imagination.

AT length Mr. Gough has accepted my invitation, and has adverted to the phenomena of the grave harmonics. Theſe ſounds, he thinks, are merely mental and imaginary; I ſuppoſe them real and material; Lagrange, whom I have already quoted, is of the ſame opinion; and while I have the authority of a man who is allowed to be either the firſt or ſecond phyſical mathematician in Europe, I ſhall be very unwilling either to diſbelieve my ears, or to confeſs that Mr. Gough has convicted me of error.

Developement of the theory by that author.

To ſhew that I have not advanced a theory ſo new as Mr. Gough has deemed it, I ſhall quote a paſſage from the firſt volume of the Miſcellanea Taurinenſia. "We have ſeen," ſays Lagrange, "that the particle of air which is found in a

* Philoſophical Journal, IV. page 1.

place

place where two ſounds meet, receives an agitation different from that which is produced by each ſound; if therefore the ſounds are of ſuch a nature that their vibrations coincide always after a certain given time, the continued and regular impreſſion of theſe compound agitations may be diſtinguiſhed from the ſimple agitations, and an ear ſufficiently exerciſed will hear a third ſound, of which the relation to the others may be found by comparing the number of ſeparate vibrations that each of them completes between two ſucceſſive coincidences," p. 103. "And the compound agitation may be conveyed to the ear in an infinite number of —— ſituations." p. 104. "We have already ſpoken of the beats of Mr. Sauveur, and we have ſeen that they correſpond exactly with the coincidences of the vibrations; there is therefore every reaſon to believe that they are formed in the ſame manner by the meeting of two ſounds. And it is probable that the third ſound of Tartini is only produced by a ſeries of theſe beats." P. 105.

Were the conteſt to be decided by authority, it is probable that your readers would prefer that of Lagrange to Mr. Gough's and mine united: but we have no occaſion for any thing more than reaſon and experiment. If the mind were capable of making up a ſound in the way that Mr. Gough ſuppoſed, we ought to hear, whenever the impulſes of one ſound biſect either accurately or very nearly, the intervals between the impulſes of another ſound, an imaginary note, an octave above the ſeparate ſounds: if, on the contrary, my opinion is true, we muſt conclude that the retrograde motions of the one will counteract the direct motions of the other, and that both the ſounds will be deſtroyed.

If the grave harmonics be the reſult of compariſon; uniſons, of which the vibrations biſect each other, ought to give the octave;—but if they reſult from coaleſcence of undulations, the ſounds muſt here deſtroy each other.

Happily the point thus at iſſue may be determined by a very ſimple experiment: we have two ſounds ſtanding in this relation, in the intervals between the beats of two muſical chords tuned very nearly in uniſon. And if we liſten to a grave ſound which beats very ſlowly, while it is dying away, we ſhall obſerve, that in the interval of the laſt and fainteſt beats, when the ſound is leaſt mixed by reflections and irregular propagations, the note, inſtead of riſing to the octave, is wholly loſt.

Experiment to ſhew that the latter fact obtains in nature.

I confeſs with pleaſure, that Mr. Gough's explanation of the reſemblance which I have pointed out between the perception

The perception of a faint ſound from a tuning fork held be-

tween the teeth will be attended with a beat if another found nearly unifon be tranfmitted through the air:

ception of a grave harmonic, and the fenfation of a ringing in the ears, is ingenious and probable; and I can mention a fingular experiment in confirmation of his opinion. If a tuning fork, faintly vibrating, be held between the teeth, and a found nearly approaching to the fame note be tranfmitted through the air, the beating will be nearly as diftinct as if both founds arrived through the fame medium. From this circumftance, however, I only infer that all founds enter the ultimate organ of hearing nearly enough in the fame direction to produce an alternate intenfion and remiffion.

whence it is inferred the ultimate direction of all founds is nearly the fame.

The grave harmonics differ from primitive founds; but they no lefs real than compound tides, &c.

I allow that fuch a found differs from primitive founds in its want of appropriate direction, and in its mode of propagation. The daily tide at Batfha in Tunquin neither comes from the eaft nor from the weft, but it is as much a real tide as the grave harmonics are real founds. If any perfon fhould infift that the phenomenon is not a tide, but an alternate elevation and depreffion of the water only, and that it exifts only in the fenfations of the obfervers, and not in the fea, I fhould be very little difpofed to enter into arguments with him on the fubject.

Remarks on the proper vehicles of philofophical papers. Philof. Tranf.—Provincial Memoirs. Journals.

With refpect to the communication of my remarks to the Manchefter Society, I beg leave to reply, that if I thought a paper of permanent importance to the extenfion of fcience, I fhould confider myfelf as bound by my duty to the Royal Society and to pofterity, to offer it for infertion in the Philofophical Tranfactions; but if it were of a mere fuperficial and temporary nature, I fhould think it fufficient to publifh it in a refpectable literary Journal. I do not mean any difrefpect to provincial focieties, but many papers have appeared in the volumes of the Manchefter Memoirs, which would perhaps have been more fuitably placed in thofe of the Philofophical Tranfactions.

Your obedient,

humble fervant,

THOMAS YOUNG.

Jan. 10, 1803.

IV. *Experiments*

IV.

Experiments and Obſervations on the Power of Fluids to conduct Heat; with Reference to Count Rumford's Seventh Eſſay on the ſame Subject. *By* John Dalton.

(*Concluded from Page* 58.)

Count Rumford's principal experiments are thoſe in which a cake of ice was confined on the bottom of a cylindrical glaſs jar, of 4.7 inches in diameter, and 14 high, and water poured upon it of different temperatures ſuffering it to ſtand, without agitation. He found that about 6lb of boiling hot water melted little more ice than as much water of 41°; and that by making ſuch allowances as the experiments ſeemed to warrant for deductions when hot water was uſed, water of 41°, or 9° above the freezing point, melted quite as much, and often more, than the hot water: From which he infers, that water. and by analogy all other fluids, do not tranſmit heat in the manner that ſolids do, but circulate it ſolely by the internal motion of their particles.

Short recapitulation of Count Rumford's experiment of ice at the bottom of heated water.

The exiſtence of this internal motion he has proved decidedly; that water of a certain temperature being of the greateſt denſity, will always take the *loweſt* place, and water either warmer or colder than that degree will aſcend. This degree of greateſt condenſation he takes on the authority of others at 40°; it appears however from the experiment related above, to be ſtill more favourable to his poſition, namely 42½°: and that water of 32° muſt aſcend till it comes to water of 53°, if it be not cooled in its progreſs, which circumſtance he admits.

The internal motion or circulation.

Upon conſidering the facts related in his experiments therefore, there are *three* cauſes which ſuggeſt themſelves as conſpiring to circulate and diffuſe the heat, by which the ice is melted.

Conſideration of the facts:

1ſt. The internal motion of the liquid, by which water of 32°, incumbent upon the ice, is perpetually aſcending into a warmer region of 53°, and warmer water of 42½° deſcending to take its place.

1. Cold water at 32° riſes to the place of 53°, and warmer of 42½° deſcends.

2d. The proper conducting power of the liquid independent of internal motion.

2. The fluid itſelf may conduct.

3d. The

3. The glafs itfelf may conduct, probably little.

3d. The conducting power of the glafs jar. But as glafs is known to be a very bad conductor of heat, it can produce no material effect in thefe experiments: for which reafon Count Rumford does not appreciate the third caufe.

Count Rumford does not allow that a cold fluid rifing into a warmer can cool it;

With refpect to the operation of the firft caufe, it will *generally* be fuppofed that *cold* water rifing into *warmer* and remaining with it, the heat is impaired, and the two reduced to a common temperature. But Count Rumford does not admit of this communication; he maintains, that the two ftill retain their proper fhare of heat, notwithftanding they are mixed together. This hypothefis of his is of no peculiar confequence as far as refpects the effect of the internal motion: for the temperature indicated by a thermometer immerfed in an equal mixture of water at 32° and 53°, would be the fame as if the water was uniformly of the temperature 42½°. But it has

whence it would follow that heat cannot by tranfmiffion downwards throw water above 42½°.

material confequences in other refpects; for, if it be admitted, it annihilates the *fecond* caufe abovementioned, and it would follow that warm water being put upon cold water above the temperature of 42½°, the heat could not in any degree be propagated downwards, unlefs by agitation, and even then, upon fubfiding, the warm part ought to rife to the top, and the cold fall to the bottom.

He has not proved this by experiment.

Thefe pofitions are fo manifeftly contradictory to common opinion, that they cannot be received without proof. But Count Rumford has not given us a fingle experiment to prove them. It feemed neceffary therefore, to clear up this point by direct experiments.

Experiment 1.

Examination by Exper. 1. Hot water poured upon cold air in a glafs veffel, the upper part gradually cooled, and the lower heated.

Took a large tumbler glafs, 3½ inches diameter, and five inches deep, and filled it half way with water of 51°, then gently filled up the reft by means of a fmall fyphon, with water of 88°; a thermometer, with its bulb and ftem detached from the frame, being previoufly immerfed to the bottom. The temperatures at the top and middle were had by gently immerfing the bulb of another thermometer into the water.

Time

Time elapsed.	TEMPERATURE at top.	in the middle.	at bottom.
——	88°	——	51°
5 min.	85	——	51
12	83	75°	56
18	80	72 +	58
30	76	——	60
40	73	——	61
50	70	——	61
60	69	——	61

(Air in the room 50°.)

Experiment 2.

The fame as before; only a circular piece of wood floated upon the furface of the water, on the centre of which the ftream of the fyphon was directed to prevent the current downwards.

Exp. 2. The fame with more precaution.

(Air in the room 55°.)

Time.	TEMPERATURE. at top.	at bottom.
Before the water was poured on	....	56°
—	116°	56 +
10 min.	105	56½
20	92	57 —
30	85	57⅓
40	80	57¼
50	77	58 +
1 h. —	75	58½
— 10	72½	58½
— 20	70	59 —
— 30	67	58½
— 40	65	58 +
— 50	63	58
2 —	62	58 —
— 15	61	57½
— 30	59½	57
3 —	57½	56 (Air 52°)
5 —	53½	53 Do.

A fimilar refult was obtained in a different way by the following.

Experiment

Experiment 3.

Exp. 3. The upper part of water heated by red hot iron; the lower part became alfo hotter.

Took an ale glafs of a conical figure, $2\frac{1}{2}$ inches in diameter and 3 inches deep; filled it with water that had been ftanding in the room, and confequently of the temperature of the air nearly—Put the bulb of a thermometer to the bottom of the glafs, the fcale being out of the water: Then, having marked the temperature, I put the red hot tip of a poker, half an inch deep into the water, holding it there fteadily about half a minute; and as foon as it was withdrawn, I dipt the bulb of a fenfible thermometer into the water about $\frac{1}{4}$ inch, when it rofe in a few feconds to 180°.

	TEMPERATURE		
Time.	*at top.*	*middle.*	*bottom.*
Before the poker was immerfed			47°
—	180°	—	47
5 min.	100	60°	$47\frac{1}{2}$
20	70	60	49
1 h. —	55	—	52

Thefe experiments all evidently agree in proving water to have a proper conducting power, independent of any internal motion. It furely will not be faid that any flight motion unavoidably made at the beginning of an experiment, could continue with a powerful effect for upwards of an hour. However, to determine this matter, I made the two following experiments.

Experiment 4.

Exp. 4. Coloured water was heated by hot colourlefs water refting upon it, without mixture.

Took the glafs tumbler of the firft experiment, and filled it half way with rain water, deeply tinged with archil; then filled it up with clear warm water, as related in the 2d experiment. The upper half was but juft perceptibly tinged by the procefs and uniformly fo; it remained for an hour not vifibly altered in this refpect, though by frequently putting the bulb of a thermometer down to the middle, the colour at laft rofe in a fmall degree.

(Air

(Air 45°)
TEMPERATURE

	at top.	middle.	bottom.
Before the warm water was poured on			44°
Time	105°	—	—
7 min.	97	—	47+
17	86	—	48
27	79	—	49+
37	75	68	50
47	70	66	50+
57	66	62	51½
1 h. 7	60	62	51½
— 17	60+	59	51½
— 27	59	—	51½

Experiment 5.

Warm coloured falt and water was cooled by cold frefh water refting on it without mixture.

A glafs tube near an inch in diameter, and 16 inches long, was half filled with a coloured folution of common falt in water, warm; a fmall thermometer was wholly immerfed in it, and cold clear water carefully poured upon the whole fo as to fill the tube; the colour afcended very little, and continued invariable after the procefs of filling. The warm folution was of courfe made of greater fpecific gravity than the cold water.

(Air 45°)
TEMPERATURE

	at top.	bottom.
Time.	45°	85°
5 min.	53	79
10	53	74
21	52	69
31	51	66
45	50½	64
58	50	61
1 h. 31	49	56½
3 30	47	51
— 55	47	50
4 15	—	49
7 5	46	48

To

To determine whether hot and cold water being ſuddenly mixed, and agitated, the hot would afterwards riſe to the top, was the object of

Experiment 6.

Exp. 6. Hot and cold water being mixed, did not afterwards ſeparate.

Air in the room 50°.—About ½ pint of water of 130° was poured into a cold tumbler glaſs, and immediately after as much water of 50°; the mixture was agitated for half a minute by a deal rod; after which an immerſed thermometer ſtood at 85°, both at top and bottom; it was then ſet by in a ſtill place for examination.

	TEMPERATURE	
Time.	*at top.*	*bottom.*
15 min.	$77\frac{1}{4}$°	77°
30	73	$72\frac{2}{3}$
45	68	$67\frac{2}{3}$
1 h. —	64.8	64.6

From all which water is concluded to be a proper conductor.

From all theſe experiments it is evident, that water has a proper conducting power: In the laſt experiment, if the particles of water during the agitation had not actually communicated their heat, the hot ones ought to have riſen to the top, and the cold ones ſubſided ſo as to have made a material difference in the temperature. It is, however, equally evident, that water is a *bad conductor* of heat, probably as it is of electricity; the deſcent of the heat in the ſecond experiment is wonderfully ſlow; a ſlight agitation for one *ſecond* would do as much to induce the equilibrium as ſtanding ſtill one *hour*. In repeating the third experiment, in a wine glaſs, I have ſeveral times known water ½ an inch deeper to differ 50° in temperature from the incumbent water.

We muſt conclude, therefore, that the quick circulation of heat in water over a fire, &c. is owing *principally* to the internal motion excited by an alteration of ſpecific gravity; but not *ſolely* to that cauſe as Count Rumford has inferred.

If it be proved that water conducts heat, it will ſcarcely be neceſſary to prove, that other fluids conduct it, and that they communicate it one to another:—The two following experiments ſhew that mercury conducts it, and that water and mercury reciprocally communicate it.

Experiment

Experiment 7.

Took a cylindrical glaſs tube, of 1 inch internal diameter, and put 1¼ inches in depth of mercury into it, and immerſed the bulb and ſtem of a thermometer to the bottom, the ſcale as uſual being above the liquid; then put 2¼ inches of warm water upon it by a ſyphon, and let it ſtand without agitation.

Exp. 7. Hot water, reſting on mercury, heated it.

TEMPERATURE.			TEMPERATURE.		
	Merc.	*Water.*	*Time.*	*Merc.*	*Water.*
Time.	56°	122	14 m.	74½°	92°
3 m.	70	118	19	73	87
6	73	110	27	71	78
11	75	100			

Experiment 8.

Into a tumbler glaſs, 2½ inches in diameter, poured an inch in depth of mercury, and heated it to 110°; upon which was poured an inch of water at 50°, and then kept ſtill.

Exp. 8. Cold water upon hot mercury, was heated.

	TEMPERATURE.	
	Merc.	*Water.*
Time.	110°	50°
4 min.	74	70
8	71	70½
10	70½	70

Finding that water was ſo bad a conductor of heat, I was deſirous to learn how ice would conduct it, and tried it as follows.

Experiment 9.

Feb. 9th. Out of a maſs of ice, by means of a hot iron, I ſhaped a cylindrical piece, 3 inches in diameter, and 5½ inches long, clean and pure; its weight 17 ounces. Made a ſmall round hole at one end, one inch deep, and the ſize of a thermometer bulb, which was incloſed in it. The other end of the piece was put into a baſon of ſnow and ſalt, to the depth of from ½ to 1½ inches, the temperature of which was kept below 10° for 1¼ hours. Air 37°.

Ice, immerſed partially in the freezing mixture, conducted very badly,—much worſe than water.

Time elapſed.	Therm. in the liquid.	Therm. in the ice.
	5°	32°
1¼ h. at a medium	7	31½

N. B.

N. B. This defcent of half a degree was gradual, but did not commence till long after the beginning of the experiment. After this the piece of ice was inclined to one fide, by which nearly one half of it was immerfed in the cooling liquid, and the inclofed bulb of the thermometer was now not more than an inch from the cold mixture.

H.	M.	Therm. in the liquid.	Therm. in the ice.
1	50*	14°	28°
2	20	19	28
2	50	22	Ice along with the therm. flipped down into the cold liquid.

The ice now weighed 12½ ounces: the reft had been liquified by the operation of the faline liquor.

This experiment, I think, decidedly proves that ice is a worfe conductor of heat than water: indeed this is not wonderful; for it is faid, that ice at a low temperature becomes an electric.

Mixture is much more effectual for equalizing temperature, becaufe hot and cold particles are brought together.

It is certainly a remarkable circumftance, but not at all inconfiftent with the known laws of heat, that in a mixture of hot and cold liquids, the uniform temperature fhould be fo foon induced by agitation and fo flowly by reft: but when we confider, that in the former cafe, hot and cold particles are brought together, and that in the latter there is a feries of particles one upon another, gradually rifing in temperature, but differing by infenfible degrees, we fhall not wonder at the facts. When any one particle of water, or any other body, has one above it, warmer by an infenfible degree, and another below it, colder by an infenfible degree, its power to tranfmit heat muft be very fmall †. Thefe confiderations gave rife to the two following experiments.

The gradual difference of temperature renders water a bad conductor.

Experiment 10.

Exp. 10. An heated mercurial thermometer expofed to air, gave out moft heat when at the higheft temperatures.

A mercurial thermometer was taken, its bulb $\frac{4}{10}$ inch in diameter, and hanging clear of the fcale: it was heated by the flame of a candle to 600°, and then laid upon a table with

* From the beginning of the experiment.

† Will not this argument apply to folids univerfally? contrary to fact. N.

the

the bulb projecting over the edge, and was thus left to cool by the mere operation of the air in the room, which was 52°. The following is the medium refult of two experiments, which, however, agreed with each other almoft in every obfervation.

Time.	*Temp.*	*Time.*	*Temp.*
0	600°	18 half m.	66°
1 half m.	450	19	64
2	350	20	62
3	280	21	60
4	229	22	59
5	195	23	58
6	168	24	57
7	145	25	56
8	128	26	55
9	115	27	54
10	104	28	53+
11	95	29	53
12	88	30	53
13	81	31	53
14	77	32	52+
15	73	33	52
16	69+	Air in the room	52
17	68		

Experiment 11.

Another thermometer, having a fimilar bulb, but a fcale with much larger degrees, was heated and cooled in the fame manner.

Time.	*Temp.*	*Time.*	*Temp.*
—	85°	16	56.3°
1 half m.	79½	17	56¼
2	75	18	56
3	71+	19	55.9
4	68½	20	55.7+
5	66¼	21	55.6+
6	64½	22	55.5
7	63	23	55.4
8	61½	24	55.3
9	60	25	55.25
10	59	26	55.2—
11	58¼	27	55.1+
12	58	28	55.1—
13	57¼	29	55.+
14	57	30	55
15	56.6		

 In

In these experiments we may consider mercury and air mixed together of unequal temperatures, with a thin partition of glass—and from the last we may conclude, that the thermometer imparted to the air 40 times more heat in half a minute, when its temperature was 30° above the air, than when it was only 1° above it.

Argument from the Count's own experiments. No water but that below 53° could descend to the ice, and consequently this only would be cooled, and the fusion of the ice would then ceafe.—Contrary to fact.

We shall now advert a little to Count Rumford's experiments. It will easily appear, that arguing fairly upon his own hypothesis he can never account for the phenomena observed: for, hot water being poured upon ice, an internal motion would take place near the surface of the ice, by which a stratum of water of a certain thickness would be reduced to 32°, and then all further reduction of the ice must cease; because all the superincumbent water being above 53° would be lighter and could not descend to the ice. But this is quite contrary to what took place. The facts, however, will admit of a satisfactory explanation upon established principles.

Explanation from the known facts. The ice is fused (1) by the proper conducting power of the water; and (2) by its motion. The first is as the heat, and the second diminishes in some unknown ratio as the temper. differs from 42½.°

By experiments 10 and 11, it appears, that the quantity of heat given out by a body, during any small given portion of time, is nearly as the excess of the temperature of the body above the cooling medium. Hence, then, we may conclude, that the effect of hot water upon ice arising from the proper conducting power of water, will be nearly as the heat of the water. What effect the other cause may produce, it will be difficult to determine from theory: experience will be the best guide. One thing, however, appears pretty certain, that its effect must be a *maximum*, when the temperature of the water at large is 42½°; because then there can never want a determination of the particles *downward* to supply the place of the lighter water of 32° ascending. If the temperature of the water exceed 42½°, then the effect of the internal motion will be less, diminishing by some unknown ratio.

Water at 42½° appears to fuse (chiefly by its motion) nearly the same as water at 190° (chiefly by its heat.)

As far as I can judge from Count R's experiments, the joint effects of those two causes should be nearly the same with water of 42° and water of 190°. Taking this, therefore, for granted, we shall be enabled to sketch a table of the values of these two causes for every 10° of temperature. The numbers expressing the effect of the proper conducting power, are derived from the 10th experiment, and consequently are not purely hypothetical: those expressing the other effect, except 42° and 192°, are put down hypothetically, because the law of decrease has not been ascertained.

It is to be ſuppoſed, that a given quantity of water, of the ſeveral temperatures mentioned, is carefully poured upon a cake of ice at the bottom of a cylindrical glaſs jar, and ſtands without agitation for a given time, as half an hour; then the proportionate quantity of ice ſuppoſed to be melted by the two cauſes ſeparately are ſtated in numbers, and then the ſums are taken to expreſs the joint effects. The reſults ſketched out for different temperatures.

Temperature

Temperature of the water on the ice.	32.°	42.°	52.°	62.°	72.°	82.°	92.°	102.°	112.°	122.°	132.°	142.°	152.°	162.°	172.°	182.°	192.°	202.°	212.°
Ice melted by the conducting power of the water.	0.	2.	3.	6.	9.	11.	12.	14.	17.	19.	21.	23.	25.	27.	30.	33.	35.	38.	40.
Ice melted by the internal motion of the particles of water.	0.	36.	35.	31.	25.	19.	16.	14.	12.	10.	8.	7.	6.	5.	4.	3.	3.	3.	3.
Total quantity melted by both causes.	0.	38.	38.	37.	34.	30.	28.	28.	29.	29.	29.	30.	31.	32.	34.	36.	38.	41.	43.

After what has been said, I need not caution my readers not to confider this table as accurate. The principle of it, however, cannot I conceive be difproved: that the operation of the conducting power muft be proportionate to a feries of numbers beginning from 0 at 32°, and gradually increafing in fome ratio with the temperature above 32°, cannot, I think, be controverted; and that the operation of the internal motion muft begin from 0 at 32°, and increafe till it arrives at its *maximum* at 42½°, and then decreafe again ever after, is alfo, I apprehend, unqueftionable: thus, when the jar had water of 42°, in Count R's experiments, this internal motion muft have had a range of 8 inches in depth; whereas, when hot water alone was ufed, it had not more than ¼ of an inch to range from the temperature of 32 to that of 53°. Obfervations on the table.

The following table exhibits a concife view of all the material varieties of Count Rumford's experiments, with their refults. Table of the varieties of the Count's experiments.

Experiment

	Temperature of the water when poured on the ice. At the beginning	end	Medium Temperature.	Water in the jar surrounded by		Ice melted in 30 minutes.
Experiment 32	41°	40°	40½°	Air	41°	617 Grains
35	189	180	184½	a warm covering of cotton wool.		747
38	41	43	42	Air	61	676
39	180	157	171½	Air	61	559
45	188	64	128	Ice & water	32	406
51	61	49	55	Ice & water	32	660
53	61	60	60½	Air	61	642

Count Rumford attempts to explain why there was lefs ice melted in fuch experiments as the 45th than in thofe like the 39th, and attributes the diminution of the effect to the defcending currents, occafioned by the cold mixture furrounding the warm one, which he thinks would obftruct the oppofite ones afcending from the ice. But the effect in the 51ft, compared with the 53d, being juft oppofite, he paffes over without explanation.—I have no doubt myfelf, but that the true caufe of the differences in both cafes, is to be found in the column expreffing the *mean temperature* of the water, and not in that expreffing its fituation, which I confider as having nothing to do in the bufinefs, but as it affects the general temperature. The *maximum* effect with cold water will be when it is of the temperature of about 48° or 50°, and the *minimum* above it probably about 100° or 120°; and in proportion as the mean temperatures, in any experiment, deviate from thofe points, the effects vary accordingly, let other circumftances be what they may.

Remarks.

The differences depend on the mean temperature of the water.

Thus I have attempted to explain the rationale of thefe very curious and interefting experiments, in a manner different to what their ingenious author has done. And muft now leave it to the reader to form his opinion.

Conclufion.

V.

A Method of examining refractive and difperfive Powers, by prifmatic Reflection. By WILLIAM HYDE WOLLASTON, *M. D. F. R. S.* *

IN examining the power with which various fubftances refract and difperfe light, I have for fome time paft employed a method unnoticed by writers on optical fubjects; and, as it is not only convenient in common cafes of refraction, but alfo capable of affording refults not attainable by other means, I have been induced to draw up a fhort account of the method itfelf, and of the moft remarkable inftances of its application.

New method of examining the refracting and difperfing powers of bodies, upon light;

This method was fuggefted by a confideration of Sir Ifaac Newton's prifmatic eye-glafs, the principle of which depends on the reflection of light at the inner furface of a denfe refracting medium.

Since the range of inclination within which total reflection

by angle of total reflection at the

* Philof. Tranf. 1802.

takes

confine of the denfer of two mediums; takes place, depends not only on the denfity of the reflecting prifm, but alfo on the rarity of the medium adjacent to it, the extent of that range varies with the difference of the denfities of the two media. When, therefore, the refractive power of one medium is known, that of any rarer medium may be learned, by examining at what angle a ray of light will be reflected from it.

General fact ftated. The angle is greater, the greater the refractive denfity of the medium towards which the light paffes; For inftance, when any object is laid under a prifm of flint-glafs, with air alone interpofed, the internal angle of incidence at which the vifual ray begins to be totally reflected, and at which the object ceafes to be feen by refraction, is about 39° 10′; but, when the object has been dipped in water, and brought into contact with the glafs, it continues vifible, by means of the higher refractive power of the water, as far as 57½° of incidence. When any kind of oil, or any refinous cement, is interpofed, this angle is ftill greater, according to until total tranfmiffion. the refractive power of the medium employed; and, by cements that refract more ftrongly than the glafs, the object may be feen through the prifm, at whatever angle of incidence it is viewed.

Fluids are eafily applied to the denfe medium (or glafs): But folids require the interpofition of a fluid more refringent than themfelves. In examining the refractive powers of fluids, or of fufible fubftances, the requifite contact is eafily obtained; but, with folids, which can in few inftances be made to touch to any great extent, this cannot be effected without the interpofition of fome fluid, or cement, of higher refractive power than the medium under examination. Since the furfaces of a ftratum fo interpofed are parallel, it will not affect the total deviation of a ray paffing through it, and may therefore be employed without rifk of any error in confequence.

Thus, refin, or oil of faffafras, interpofed between plate glafs and any other prifm, will not alter the refult.

A triangular glafs prifm may be ufed for comparing the powers of two bodies; If, on the fame prifm, a piece of felenite and another of plate-glafs be cemented near each other, their powers may be compared with the fame accuracy as if they were both in abfolute contact with it.

For fuch a mere comparifon of any two bodies, a common but a fquare prifm fhews the fine of refraction without calculation. triangular prifm is beft adapted; but, for the purpofe of actual meafurement of refractive powers, I have preferred the ufe of a fquare prifm, becaufe, with a very fimple apparatus, it fhows the fine of refractive power fought, without the need of any calculation.

Figure and application of the fquare prifm. Let A, Fig. 1, Plate IV. be a fquare or rectangular prifm, to which

which any fubftance is applied at b, and let any ray of light parallel to $c\,b$ be refracted through the prifm, in the direction $b\,d\,e$.

Then, if $e\,f$ and $e\,d$ be taken proportional to the fines that reprefent the refractive powers of the prifm and of air, $f\,g$, which is intercepted between f and the perpendicular $e\,g$, will be the correfponding fine to reprefent the refractive power of the medium b. For, fince $e\,d\,g$ (oppofite to $e\,f$) is the angle of refraction, $e\,f\,g$ (oppofite to $e\,d$) muft be equal to the angle of incidence $b\,d\,h$; and $e\,f : f\,g :: b\,d : d\,h ::$ fine of $c\,b\,i$: fine of $h\,b\,d$. Conftruction to fhew the fine of refraction from any given rarer body into glafs.

All therefore that is requifite for determining the refractive power of h, is to find means of meafuring the line $f\,g$. On this principle, the inftrument in the annexed fketch (Fig. 2.) is conftructed. On a board $a\,b$ is fixed a piece of flat deal $c\,d$, to which, by a hinge at d, is jointed a fecond piece $d\,e$, 10 inches long, carrying two plane fights at its extremities. At e is a fecond hinge, connecting $e\,f$, 15,83 inches long; and a third at the other extremity of $e\,f$, by which $f\,g$ is connected with it. At i alfo is a hinge, uniting the radius $i\,g$ to the middle of $e\,f$; and then, fince g moves in a femicircle $e\,g\,f$, a line joining e and g would be perpendicular to $f\,g$. Inftrument giving the fine by infpection.

The piece $c\,d$ has a cavity in the middle of it, fo that, when any fubftance is applied to the middle of the prifm Γ, it may continue to reft horizontally on its extremities. When $e\,d$ has been fo elevated that the yellow rays in the fringe of colours (obfervable where perfect reflection terminates) are feen through the fights, the point g, by means of a vernier which it carries, fhows by infpection the length of the fine of refraction fought.

The advantages which this method poffeffes above the ufual mode of examining refractive powers, are greater than they may at firft fight appear. The ufual practice has been, to form two furfaces of the fubftance under examination, fo inclined to each other that the deviation occafioned by them might be meafured. The inclination of thefe furfaces to each other muft alfo be known; and thence the refractive power might be computed. But, in the method here propofed, it is fufficient to have only one furface, and the refult is obtained at once, without computation. This method is moft accurate and eafy: it requires only one furface and gives the refult at fight.

The facility of determining refractive powers, is confequently fuch as to render this property of bodies a very convenient teft in many philofophical inquiries. For difcovering the purity of effential oils, fuch an examination may be of con- Its facility renders it ufeful as a teft in philofophical inquiries.

 fiderable

fiderable utility, on account of the fmallnefs of the quantity requifite for trial. In oil of cloves, for inftance, I have met with a wide difference. The refractive power of genuine oil of cloves, is as high as 1,535; but I have alfo purchafed oil by this name, which did not exceed 1,498, and which had probably been adulterated by fome lefs refractive oil.

It is applicable to opake bodies.

For fuch purpofes, the refractive power of opaque fubftances may often be deferving of inquiry, which could not be learned by any means at prefent in ufe. For, in the ufual mode, a certain degree of tranfparency is abfolutely neceffary; but, for trial by contact, the moft perfect opacity does not occafion the leaft impediment.

Inftance.

Among other inftances in which I have taken advantage of this circumftance, I may mention a fubftance that had been found in one of the iflands of the North Pacific Ocean, which, to all outward appearance and by various trials, feemed to be perfect bees-wax, although it is fuppofed that there are no bees in the ifland from which it was brought. On placing it by the fide of a piece of bees-wax, in contact with a prifm, the perfect equality of their refractive powers afforded a ftrong confirmation of the opinion before formed of their identity.

Bodies of varying denfity may be examined by it,

For the examination alfo of media of which the refractive denfity is not uniform, the general method of trial by deviation wholly fails; on the contrary, by placing a varied medium in contact with a prifm, all its gradations of denfity, from greateft to leaft, become at once the object of mere infpection. An inftance of this may very readily be feen with a piece of gum, the furface of which has been moiftened for a few minutes; when, by clofe application to a prifm, a refractive power may be difcerned, varying from that of the water on the furface, 1,336, to nearly 1,51, the refractive power of gum arabic.

particularly the cryftalline lens.

I fhould not fo much infift on this advantage, were it not for the opportunity hereby afforded of examining the cryftalline lens of the eye, which is now known to be generally more denfe in the centre than at its furface.

Mr. Haukfbee, who was not aware of this difference, has eftimated the refractive power of the cryftalline lens, by forming it into a wedge by plates of glafs, fomewhat higher than I find it to be; but, with his accuftomed accuracy, he remarked the apparent enlargement of an object, occafioned by the variations of its denfity, which he was unable to explain.

In the table that follows, I have fet down, not only the limits of

of refractive power in a cryſtalline lens of an ox, aſcertained by trial, but alſo an average, computed from the refractive denſity of a dried cryſtalline of an ox, of which the weight had been firſt taken in the recent ſtate, and the quantity of water loſt by drying alſo meaſured.

Table of refractive powers.

The table exhibits a ſeries of ſubſtances, arranged according to their refractive powers. That of the diamond is copied from Sir Iſaac Newton; of other bodies to which (on account of their being more denſe than glaſs) the machine for meaſurement would not apply, the refractive powers have been found by other means, for the ſake of furniſhing a more continued ſeries of ſubjects for comparative experiments. The reſt have been compared by this method; and their power, when expreſſed in numbers, actually meaſured.

TABLE I.

Diamond	2,44	Oil of ſaſſafras	1,536
Plumbago	—	Red ſealing-wax	—
Native ſulphur (double)	2,04	Spermaceti, cold	—
Glaſs, conſiſting of lead 6 and ſand 1	1,987	Sugar, after fuſion	—
Glaſs of antimony	1,98	Arſeniate of potaſh	—
Jargon	1,95	Maſtic	—
Spinelle ruby	1,812	Elemi	—
Arſenic	1,811	White wax (cold)	—
Muriate of antimony,	variable	Oil of cloves	1,535
White ſapphire	1,768	Copal	1,535
Gum dragon	—	Anime	1,535
Iceland ſpar, ſtrongeſt	1,657	*Rhdcliffe crown glaſs*	1,533
Sulphate of barytes (double)	1,646	Pitch	—
Balſam of Tolu	1,60	Centre of cryſtalline of fiſh, and dry cryſtalline of an ox	1,530
Guaiacum	1,596	Canada balſam	1,528
Benzoin	—	*Crown glaſs, common*	1,525
Flint glaſs	1,586	Selenite	1,525
Ditto	1,583	Caoutchouc	1,524
Horn	—	Gum lac	—
Phoſphorus	1,579	*Dutch plate glaſs*	1,517
Mica	—	Human cuticle	—
Opium	—	Gum arabic	1,514
Amber	1,547	Balſam of capivi	1,507
Rock cryſtal (double)	1,547	Oil of amber	1,505
Old plate glaſs	1,545	*Engliſh plate glaſs*	1,504
Colophony	1,543	*French plate glaſs*	1,500
Box-wood	—	Oil of nutmeg	1,497
Bees-wax	1,542	Sulphate of potaſh	1,495

Tallow

Tallow, cold	1,49	Cryſtalline lens of an ox	1,447
Iceland ſpar, weakeſt	1,488	to	1,380
Camphor	1,487	Computed average of ditto	1,430
Linſeed oil	1,485	Sulphuric acid	1,435
Butter, cold	1,480	Fluor ſpar	1,433
Eſſence of lemon	1,476	Nitric acid (ſp. gr. 1,48)	1,410
Oil of turpentine, common	1,476	Alcohol	1,37
——— rectified	1,470	White of an egg	1,36
Oil of almonds	—	Æther	1,358
——— olives	1,469	Vitreous humour of an eye	1,336
——— peppermint	1,468	Water	1,336
——— lavender	1,467	Atmoſpheric air (Haukſbee)	1,00032
Tallow, *melted*	1,460		
Alum	1,457		
Spermaceti, *melted*	1,446		

ON THE DISPERSION OF LIGHT.

This method is likewiſe applicable to ſhew the diſperſion of light.

The method above deſcribed for inveſtigating refractive powers, may alſo be employed with ſimilar advantage for inquiries into the diſperſion of light by different bodies, and the conſequences that reſult from their combined action.

The limit of perfect reflection is marked by a fringe of colours;

When a glaſs priſm is placed in contact with water, and brought near the eye, in ſuch a poſition that it reflects the light from a window, the extent of perfect reflection is ſeen to be bounded by a fringe of the priſmatic colours, in the order of their refrangibility.* The violent rays, being in this caſe the moſt refrangible, appear ſtrongeſt and loweſt, on account of the leſs obliquity that is requiſite for their reflection.

except when (the refractions being unequal) the diſperſions are equal.

But it may happen that two media, which refract unequally at the ſame incidence, may diſperſe equally at that incidence. Under theſe circumſtances, a pencil of rays paſſing from one of ſuch media into the other, will be refracted, without diſperſion of its colours. The boundary of priſmatic reflection would then be found a well defined line, free from colour, if the ſurface at which the reflected light emerges from the priſm, were at right angles to its courſe.

If the diſperſion by the rarer medium be greateſt the order of colours will be inverted.

When the diſparity of the diſperſive powers of the media is ſtill greater, it may alſo happen, that the uſual order of priſmatic colours will be reverſed; and then the red will appear ſtrongeſt and loweſt in the fringe, unleſs the colours ſo pro-

* Newton's Optics. Book i. part 2. Exp. 16.

duced

duced are counteracted by refraction at their emergence from the prifm.

An inftance in which the colours are fo reverfed, may be feen by application of oil of faffafras to a prifm of flint glafs. Inftance: F. glafs and ol. faffafras.

So high is the difperfive power of this oil, that, in refractions from flint glafs into it, the red rays are refracted more than the violet.

It muft be obferved that, in this experiment, when the angle of reflection within a triangular prifm exceeds 60°, the angle of emergence is fuch as would alone occafion the red rays to appear lowermoft; but, when the glafs ufed is rectangular, the refraction at emergence has an oppofite effect; any reverfion of colour will therefore be in fome degree corrected, and may not be feen, unlefs the difperfive power of the medium in contact much exceeds that of the glafs.

A cafe of refraction with an inverted order of colours, has been obferved by Dr. Blair,* in a compound object-glafs where crown-glafs was in contact with oil of turpentine. From trials with lenfes, he likewife inferred, that feveral other fluids have the fame effect, when applied to that glafs. Crown glafs and oil of turpentine.

With this glafs, and alfo with plate-glafs, I have tried oil of turpentine, and many other fluids that afford a fimilar reverfion of colours, as linfeed-oil, olive-oil, the effential oils of bergamot, lemon, lavender, pennyroyal, and peppermint, ftrong nitric acid, and many artificial compounds that I fhall prefently have occafion to mention. other oils, &c.

The difperfive power of fluor fpar is the leaft of any fubftance yet examined; fo that, although its refractive power is alfo remarkably low, (confidering its great fpecific gravity,) a prifm of fluor, in contact with water or alcohol, fhows the prifmatic colours to be refracted in an inverted order. Fluor fpar difperfes very little,

With heavy fpar, the inftances of reverfion are very numerous, as its difperfive power is low, and is accompanied with great refractive denfity. In the refractions from this fpar into flint glafs, and into all oils or refins, I believe, without exception, the colours are feen reverfed. alfo ponderous fpar

Rock cryftal likewife difperfes fo little, that it exhibits the colours reverfed, when it is in contact with many fubftances of lefs refractive power than itfelf. I have tried it with Dutch plate-glafs, with Canada balfam and balfam of capivi, with and rock cryftal.

* Edinb. Tranf. Vol. III.

many

many oils effential and expreffed, and have found the colours in all thefe cafes reverfed.

Metallic falts difperfe much.

By folutions of metallic falts, a great variety of fuch appearances may be produced. Moft of thefe compounds have a highly difperfive power; and many of them may be rendered fufficiently denfe to occafion reverfion, even when applied to flint-glafs. In a more dilute ftate, they may be ufed with crown-glafs, or plate-glafs, to produce the fame effect. And fince, when further diluted by a lefs difperfive medium, they will alfo prefent an appearance of colourlefs refraction, we may, by examining the degree of dilution neceffary for that purpofe, compare the difperfive powers of any ingredients contained in them, and may gradually extend our knowledge of this property to the elements of any bodies, however compounded.

Experiments of diluting them till no colour appeared.

As a fpecimen of the method, I have in this way compared a few folutions of metals, and of other fubftances, that were each diluted till the limit of reflection appeared void of colour, when they were in contact with a rectangular piece of plate-glafs; and, in the table which follows, I have expreffed their refractive powers in that ftate of dilution, as nearly as the eye can difcern the difappearance of colour.

TABLE II.

Table of their denfities in that ftate.

	In Water.	In Alcohol.
Nitro-muriate of gold - - -	1,364	1,390
Nitro-muriate of platina - -	1,370	
Nitrate of iron - - - -	1,375	
Sulphuret of potafh - - -	1,375	
Red muriate of iron - - -	1,385	
Nitrate of magnefia		
Nitric acid - - - - -	1,395	
Nitrate of jargon		
Balfam of Tolu - - - - -	—	1,400
Acite of litharge (extract of lead) -	1,400	
Nitrate of filver		
Nitrate of copper		
Oil of faffafras - - - - -	—	1,405
Muriate of antimony - - -	—	1,410
Nitrate of lime - - - - -	1,410	1,422
Nitrate of zinc		
Green muriate of iron - - -	1,415	
Muriate of magnefia - - -	1,416	
——— of lime - - - - -	1,425	1,440
——— of zinc - - - - -	1,425	
Effence of lemon - - - -	—	1,430
Balfam of capivi - - -	—	1,440

It

It may here be feen, that feveral of the metals increafe the difperfive powers of nitric and muriatic acids, and confequently exceed them in that refpect. Of all thefe fubftances that I have yet tried, gold and platina are the moft difperfive. The leaft difperfive of the metals is zinc.

Gold and platina moft increafe the difperfion; zinc leaft.

The earths alfo are found to poffefs this property in very different degrees: that of the jargon and magnefia differ but little from nitric acid in difperfive power; but filiceous earth, on the contrary, is inferior to water.

The earths differ much.

By comparing the falts formed with the nitric and muriatic acids, it appeared probable that the former had the higher difperfive power; but a more direct comparifon could not be made by means of the rectangular piece of plate-glafs, as muriatic acid could not be rendered fufficiently denfe for fuch a trial; I therefore made ufe of a triangular prifm of crown-glafs, which is in itfelf lefs difperfive than any plate-glafs, and, from the relative pofition of its furfaces, occafioned lefs correction of the colours. With this prifm, I found that ftrong muriatic acid (having a refractive power 1,394) exhibited the colours reverfed; and that, when it was diluted till the limit of reflection appeared void of colour, its refractive power was reduced to 1,382. But the difperfive power of nitric acid, when tried by the fame prifm, proved to be greater; for this acid required to be diluted till its refractive power did not exceed 1,375, before the colour was wholly deftroyed.

Nitric acid difperfes more than muriatic.

In the table it may be obferved, that the red and green muriates of iron, though confifting of the fame metal and acid, differ very much in difperfive power; and, confequently, that fome caution will be neceffary, in attempting to compare the different metals with each other by means of the falts containing them, as any difference obferved may be owing in part to a difference in the quantity of acid to which they are united, and in part to their different proportion of oxygen.

More oxygen feems to produce more difperfion in the muriate of iron;

A ftriking inftance of the latter is manifeft, from a comparifon of fulphur with the fulphuric acid; for, while the former appears to exceed the metallic oxides in difperfive power, the latter is inferior even to water.

but lefs in the oxidation of fulphur.

As I have likewife, at various times, made many experiments on difperfion by means of wedges, in a manner nearly fimilar to that employed by Mr. Dollond, Dr. Blair, and others, I have

The order of various bodies as to their difperfive powers;

I have endeavoured to reduce the ſeveral ſubſtances thus examined to one table; but, as the limits of colour are in few inſtances ſufficiently well defined for accurate menſuration, I have not attempted to add any numerical eſtimate of their powers, but have merely aſcertained the order in which thay ſucceed each other; and, in the following table, have arranged them according to the exceſs of their effect on violet above red light, at a given angle of deviation.

tabulated with their refractive powers annexed.

TABLE III.

Order of diſperſive Powers.	Refr. Power.	Order of diſperſive Powers.	Refr. Power.
Sulphur - -	2,04	Amber - -	1,547
Glaſs of lead ($\frac{1}{7}$ ſand)	1,987	Diamond - -	2,44
Balſam of Tolu -	1,60	Alum - -	1,457
Oil of ſaſſafras -	1,536	Plate-glaſs, Dutch	1,517
Muriate of antimony		Ditto, Engliſh -	1,504
Guaiacum - -	1,596	Crown glaſs -	1,533
Oil of cloves -	1,535	Ruby (ſpinelle) -	1,812
Flint-glaſs - -	1,586	Water - -	1,336
Colophony - -	1,543	Sulphuric acid -	1,435
Canada balſam -	1,528	Alcohol - -	1,37
Oil of amber -	1,505	Sulphate of barytes	1,646
Jargon - -	1,95	Selenite - -	1,525
Oil of turpentine -	1,47	Rock cryſtal - -	1,547
Copal - -	1,535	Sulphate of potaſh -	1,495
Balſam of capivi -	1,507	White ſapphire -	1,768
Anime - -	1,535	Fluor ſpar - -	1,433
Iceland ſpar -	1,657		

Numerous achromatic combinations.

By compariſon of this table with the order of refractive powers, as contained in the firſt table, it will be ſeen how little correſpondence there is between them; and, accordingly, how numerous are the combinations by means of which a pencil of rays that paſſes through two media, may be made to deviate without diſperſion of its colours.

White light does not appear to be ſeparable by the priſm into *ſeven* nor into *three* colours.

I cannot conclude theſe obſervations on diſperſion, without remarking that the colours into which a beam of white light is ſeparable by refraction, appear to me to be neither 7, as they uſually are ſeen in the rainbow, nor reducible by any means (that I can find) to 3, as ſome perſons have conceived; but that, by employing a very narrow pencil of light, 4 primary diviſions of the priſmatic ſpectrum may be ſeen, with a degree of diſtinctneſs that, I believe, has not been deſcribed nor obſerved before.

If a beam of day-light be admitted into a dark room by a crevice $\frac{1}{20}$ of an inch broad, and received by the eye at the distance of 10 or 12 feet, through a prism of flint-glass, *free from veins*, held near the eye, the beam is seen to be separated into the four following colours only, red, yellowish green, blue, and violet; in the proportions represented in Fig. 3. Experiments by which the colours are shewn to be four.

The line A that bounds the red side of the spectrum is somewhat confused, which seems in part owing to want of power in the eye to converge red light. The line B, between red and green, in a certain position of the prism, is perfectly distinct; so also are D and E, the two limits of violet. But C, the limit of green and blue, is not so clearly marked as the rest; and there are also, on each side of this limit, other distinct dark lines, *f* and *g*, either of which, in an imperfect experiment, might be mistaken for the boundary of these colours. Remarkable facts relating to the spectrum.

The position of the prism in which the colours are most clearly divided, is when the incident light makes about equal angles with two of its sides. I then found that the spaces AB, BC, CD, DE, occupied by them, were nearly as the numbers 16, 23, 36, 25. Spaces occupied by the colours;

Since the proportions of these colours to each other have been supposed by Dr. Blair to vary according to the medium by which they are produced, I have compared with this appearance, the coloured images caused by prismatic vessels containing substances supposed by him to differ most in this respect, such as strong but colourless nitric acid, rectified oil of turpentine, very pale oil of sassafras, and Canada balsam, also nearly colourless. With each of these, I have found the same arrangement of these four colours, and, in similar positions of the prisms, as nearly as I could judge, the same proportions of them. nearly the same in various bodies;

But, when the inclination of any prism is altered so as to increase the dispersion of the colours, the proportions of them to each other are then also changed, so that the spaces AC and CE, instead of being as before 39 and 61, may be found altered as far as 42 and 58.* but is changed with the inclination.

H 2 By

* Although what I have above described comprises the whole of the prismatic spectrum that can be rendered visible, there also pass on each side of it other rays, whereof the eye is not sensible. From Dr. Herschel's experiments (Phil. Trans. for 1800) we learn, that on Invisible heat making rays on the confine of red;

Curious effects of the refraction of candle light;

By candle-light, a different set of appearances may be distinguished. When a very narrow line of the blue light at the lower part of the flame is examined alone, in the same manner, through a prism, the spectrum, instead of appearing a series of lights of different hues contiguous, may be seen divided into five images, at a distance from each other. The 1st is broad red, terminated by a bright line of yellow; the 2d and 3d are both green; the 4th and 5th are blue, the last of which appears to correspond with the division of blue and violet in the solar spectrum, or the line D of Fig. 3.

and the light of electricity.

When the object viewed is a blue line of electric light, I have found the spectrum to be also separated into several images; but the phenomena are somewhat different from the preceding. It is, however, needless to describe minutely, appearances which vary according to the brilliancy of the light, and which I cannot undertake to explain.

on one side there are invisible rays occasioning heat, that are less refrangible than red light; and on the other I have myself observed, (and the same remark has been made by Mr. Ritter,) that there are likewise invisible rays of another kind, that are more refracted than the violet. It is by their chemical effects alone that the existence of these can be discovered; and, by far the most delicate test of their presence is the white muriate of silver.

and disoxygenating rays on the confine of violet.

To Scheele, among many valuable discoveries, we are indebted for having first duly distinguished between radiant heat and light; (*Traité de l'Air et du Feu*, § 56, 57;) and to him also we owe the observation, that when muriate of silver is exposed to the common prismatic sprectrum, it is blackened more in the violet than in any other kind of light. (§ 66.) In repeating this experiment, I found that the blackness extended not only through the space occupied by the violet, but to an equal degree, and to about an equal distance, beyond the visible spectrum; and that, by narrowing the pencil of light received on the prism, the discoloration may be made to fall almost entirely beyond the violet.

It would appear therefore, that this and other effects usually attributed to light, are not in fact owing to any of the rays usually perceived, but to invisible rays that accompany them; and that, if we include two kinds that are invisible, we may distinguish, upon the whole, six species of rays into which a sun-beam is divisible by refraction.

VI. An

VI.

An Account of Dr. YOUNG'S *Harmonic Sliders. From his Paper in the Journals of the Royal Inſtitution, p.* 261.

THE combination of undulations, however cautiouſly the world may adopt its application to the explanation of optical phenomena, is of acknowledged utility in illuſtrating the phenomena of muſical conſonances and diſſonances, and of undeniable importance in accounting for many of the phenomena of the tides. Each tide is an undulation on a large ſcale; and, ſuppoſing the general form of the ocean, in conſequence of the attraction of a diſtant body, to coincide with that of an oblong ſpheroid, as it is found by calculation to do, the ſection of the ſurface of each tide, if conceived to be unbent from the circular form and extended on a plane, would form the harmonic curve. (Young's Syllabus, IV. 151. 155.) It is remarkable that the motions of the particles of the air in ſound have been generally ſuppoſed in theory to correſpond with the ordinates of this ſame curve, and that there is alſo experimental reaſon to believe, that the pureſt and moſt homogeneous ſounds do in fact agree very nearly with the law of this curve. It is therefore by far the moſt natural as well as the moſt convenient to be aſſumed, as repreſenting the ſtate of an undulation in general; and the name of theſe harmonic ſliders is very properly deduced from the harmonic curve.

Utility of the doctrine of combined undulations in explaining cotemporary ſounds; and the tides.

The harmonic ſliders

By means of this inſtrument, the proceſs of nature, in the combinations of motion which take place in various caſes of the junction of undulations, is rendered viſible and intelligible, with great eaſe, in the moſt complicated caſes. It is unneceſſary to explain here, how accurately both the ſituations and motions of the particles of air, in ſound, may be repreſented by the ordinates of the curve at different points: it is ſufficient to conſider them as merely indicating the height of the water conſtituting a tide, or a wave of any kind, which exiſts at once in its whole extent, and of which each point paſſes alſo in ſucceſſion through any given place of obſervation. We have then to examine what will be the effect of two tides, produced by different cauſes, when united. In order to repreſent this effect, we muſt add to the elevations or depreſſions in conſequence of the firſt tide, the elevations or depreſſions

viſibly exhibit the proceſs of nature in the junction of undulations.

Explanation applied to tides.

preffions in confequence of the fecond, and fubtract them when they counteract the effect of the firft: or we may add the whole height of the fecond above any given point or line, and then fubtract, from all the fums, the diftance of the point affumed below the medium.

Exhibition of a fimple tide,

To do this mechanically is the object of the harmonic fliders. The furface of the firft tide is reprefented by the curvilinear termination of a fingle board, Plate VI. Fig. 1. The fecond tide is alfo reprefented by the termination of another furface; but, in order that the height at each point may be added to the height of the firft tide, the furface is cut tranfverfely into a number of feparate pieces or fliders, which are confined within a groove or frame, and tightened by a fcrew, Fig. 2. Their lower ends are fituated orginally in a right line; but, by loofening the fcrew and moving the fliders, they may be made to affume any other form: thus they may be applied to the furface reprefenting the firft tide; and if the fimilar parts of each correfpond, Fig. 3, the combination will reprefent a tide of twice the magnitude of the fimple tides.

and of combinations which either increafe, or modify, or deftroy each other.

The more the correfponding parts are feparated, the weaker will be the joint effect, Fig. 4; and, when they are furtheft removed, the whole tides, if equal, will be annihilated, Fig. 5. Thus, when the general tide of the ocean arrives by two different channels at the fame port, at fuch intervals of time that the high water of one would happen at the fame inftant with the low water of the other, the whole effect is deftroyed, except fo far as the partial tides differ in magnitude. The principle being once underftood, it may eafily be applied to a multiplicity of cafes: for inftance, where the undulations differ in their dimenfions with regard to extent. Thus, the feries of fliders being extended to three or four alternations, the effect of combining undulations in the ratio of 2 to 1 of 3 to 1, of 2 to 3, of 3 to 4, may be afcertained, by making a fixed furface, terminating in a feries of curves, that bear to thofe of the fliding furface the ratio required: and, by making them differ but flightly, the phenomenon of the beating of an imperfect unifon in mufic may be imitated, where the joint undulation becomes alternately redoubled and evanefcent. In Fig. 6, the proportion is that of 17 to 18, and the curvilinear outline reprefents the progrefs of the joint found from the greateft degree of intenfity to the leaft, and a little beyond it.

The principle is applicable to a variety of cafes,

undulations differing in extent.

The beat of an imperfect unifon.

VII. *Obfervations*

VII.

Obſervations on the Appearances produced by the Colliſion of Steel with Hard Bodies. By Mr. DAVY, *Lecturer on Chemiſtry at the Royal Inſtitution* *.

I. MR. HAWKSBEE long ago ſhewed, that no ſparks could be produced by the colliſion of flint and ſteel † in the exhauſted receiver of an air pump, and that in this caſe a faint light only was perceived. And, ſince his time, the ſame obſervation has been very often made. Hawkſbee's exp. of flint and ſteel in vacuo.

The developement of the theory of combuſtion has clearly ſhown that the vivid ſparks obtained from ſteel in the atmoſphere, are owing to the combination of the ſmall abraded and heated metallic particles with oxigen. But it has been a matter of doubt whether, in the experiment made in vacuo, the faint luminous appearance is owing wholly to the light produced by the fracture and abraſion of the parts of the flint, or only partly to this cauſe, and partly to the ignition of the minute filaments ſeparated from the ſteel. Sparks thus produced in air, are owing to combuſtion of the ſteel.

II. I have often found, that when a fine and thin flint, which may be eaſily broken, is uſed for the colliſion in vacuo, the light is much more vivid than when a thick and ſtrong one is employed: and with a ſtrong flint, but juſt ſharp enough to give ſparks with ſteel in the atmoſphere, it is ſeldom that any light at all is produced in the exhauſted receiver. Theſe facts ſeem to ſhew that the abraded particles of ſteel are not rendered at all luminous by colliſion, except in conſequence of combuſtion; and the opinion is almoſt fully proved by the following experiment, which was made in the courſe of a lecture on the properties of light, in the theatre of the Royal Inſtitution, and which has been ſince often repeated. Light in vacuo is moſt vivid with a thin ſharp flint. The colliſion does not make the ſteel red hot.

III. A thin piece of iron pyrites ‡ (ſulphuret of iron) was inſerted in a gunlock in the place of the flint. It gave by Pyrites inſtead of flint, gave no light in vacuo.

* Journal of the R. I. 264.

† Philoſophical Tranſactions, Vol. XXIV. p. 2165.

‡ The etymology of the name of this ſubſtance ſhews that its property of giving fire by colliſion was very anciently known. It was uſed in the old gunlocks, with the revolving wheel, for inflaming the priming.

colliſion

collision in the atmosphere very vivid sparks; which were chiefly white, from the combustion of the particles of the steel; but sometimes mixed with a few red sparks from the combustion of the particles of the pyrites. The gunlock was introduced under the receiver of an air pump, and the exhaustion was made till the mercury in the short gage stood at about $\frac{6}{10}$ of an inch. The lock was then snapped, but no light whatever was perceived; and the phenomenon was uniform, every precaution being taken to render the room dark, and to preserve the apparatus in order.

Question. Can steel burn in air if not first white hot?

IV. It is well known that in common cases the finest steel wire does not burn with a white light or sparks in the atmosphere, unless it have been previously heated to a degree much above that of the red heat; it consequently at first view appears extraordinary, that the particles separated from the gunlock should be heated so as to burn vividly in air, and yet not so as to appear ignited in vacuo; for it is not easy to conceive that they emit light, which from the minuteness of their volume * cannot be perceived; or to suppose that the opacity of the metallic substances should hinder light generated at their points of contact from being visible. I had formerly supposed, in reasoning upon the phenomenon of the collision of flint and steel, that † heat and light might in common instances be only accidentally coexistent; and that in certain cases very high temperatures might be produced without causing the appearance of light. At present however I am inclined to believe, that the phenomena may be adequately accounted for upon principles that coincide with the common facts relating to the production and communication of heat.

If it cannot, why is not this white heat seen in vacuo?

Explanation. Oxidation at temperatures far below ignition, may develope more heat than can be conducted off, and may produce vivid combustion;

Mr. Stodart ‡ has shewn, that when steel is gradually heated it begins to change colour at about 430° Fahrenheit, And this change of colour is occasioned by its combination with oxigen, and, as there is every reason to believe, must be connected with the evolution of heat. At about 600°, a temperature much below that of ignition, it oxidates rapidly, and becomes covered with a bluish grey coating. And though in these cases of oxidation the heat evolved at the surface of the

* Or the short time of its emission.—N.

† Nicholson's Journal, 4to, Vol. III. p. 517.

‡ Nicholson's Journal, 4to, Vol. IV. p. 130.

the metal is not fufficient to raife the temperature of fteel wire, or a fteel plate, fo as to caufe it to enter into the vivid combuftion, yet in acting upon fuch a minute filament as that ftruck off in the gunlock, it may be fufficient to keep up the procefs of oxidation till it becomes fo vivid as to occafion the ftrongeft heat and light. Befides, the furface of this filament is very great as compared with its bulk, and the oxide produced upon it is lefs likely to form a coat which might defend the interior parts from the action of the air *. In cafe the furface be very great in proportion to the mafs.

It would not be difficult to find many analogous inftances, in which the progrefs of oxidation is dependent upon the mafs of the combuftible body, or rather upon the relation of this mafs to furface; thus, a very thin and fmall bit of phofphorus will inflame fpontaneoufly, and burn with the vivid light when wrapped up in filaments of fine cotton; whilft a thicker and larger piece will only fhine with the feeble blue light: and though a large mafs of zinc may be melted in the atmofphere without inflaming, a fmall and thin fhaving will burn vividly long before it is heated to the temperature of fufion. Other inftances. Phofphorus and zinc readily take fire in fmall maffes.

V. In confidering the general phenomena of the production of heat and light, by mechanical means, it is difficult to conceive that any confiderable increafe of temperature can be produced on a metallic furface by a fingle collifion; for the conducting power of the metals is fuch as would fpeedily caufe the heat to be communicated to the contiguous parts; and even in the cafe of the abrafion of minute particles, though the time required for their feparation from the mafs is to us imperceptible, yet it muft be fufficient to enable them to give out to it a portion of their heat. Not probable that much heat fhould be produced by a fingle collifion.

The bodies that become luminous by being ftruck or rubbed together in vacuo, or in gafes that do not contain oxigen, or under water, fuch as fluate and carbonate of lime, filiceous ftones, glafs, fugar, and many of the compound falts, are both electrics *per fe*, and phofphorefcent fubftances; fo that the flafhes they produce are moft likely occafioned, partly by Luminous appearances, from electricity or phofphorefcence, by friction.

* In turning very fine work of fteel in the lathe, fo as to afford fhavings or threads much thinner than one-thoufandth of an inch, I found that this metallic wool very readily caught fire at a candle, and burned throughout in quantities of a cubic inch or more: But it was fcarcely fo much oxided as to have loft its flexibility after this combuftion.—N.

the

the electricity excited on their surfaces by the friction, and partly by their phosphorescence, which is generally occasioned by moderate degrees of heat. It is not however improbable that in some cases, by the collision of very hard stony bodies, which are bad conductors of heat, there may be an actual ignition of abraded particles; and the supposition is countenanced by various facts. Mr. T. Wedgwood found that a piece of window glass, when brought in contact with a revolving wheel of grit, became red hot at its point of friction, and gave off luminous particles which were capable of inflaming gunpowder and hidrogen gas *. And we are informed by a late voyager †, that the natives of Oonalashka light their fires by striking together two pieces of quartz, their surfaces being previously rubbed with native sulphur, over dry grass.

Cases of actual ignition:

VIII.

Description of a Blow-pipe by Alcohol, having a safety Valve, with other Advantages. Constructed by Mr. Benjamin Hooke, *Fleet Street.*

To Mr. NICHOLSON.

SIR,

Blow-pipe with a safety valve and only one flame.

I HAVE taken the liberty of troubling you with a drawing and description of the *blow-pipe by alcohol* as I make it, which perhaps possesses the following advantages over that described in your number of September last; *viz.* Being furnished with a safety valve, to prevent accidents; having only one lamp (the wick of which being pretty large, answers both for heating the alcohol and for affording a strong blast when drawn through it); and I think superiority as to form and appearance.

Should you esteem it worthy a place in your valuable Journal, the insertion will oblige,

SIR,

Your very humble servant,

BENJAMIN HOOKE.

159, *Fleet Street,*
Nov. 20, 1802.

* Phil. Transf. 1792, p. 45.

† Sauer's Account of Billings's Expedition to the northern parts of Russia, p. 159.

A is

A is a hollow ſphere * for containing alcohol, reſting upon a ſhoulder in the ring O, Plate V. Deſcription.

B is a bent tube with a jet at the end, to convey the alcohol in the ſtate of vapour into the flame at Q, this tube is continued in the inſide up to C, which admits of A being filled nearly, without any alcohol running over.

D is a ſafety valve, the preſſure of which is determined at pleaſure, by ſcrewing higher or lower on the pillar E, the two milled nuts F and G carrying the ſteel arm H, which reſts on the valve.

I is an opening for putting in the alcohol.

K is the lamp, which adjuſts to different diſtances from A, by ſliding up or down the two pillars L. The diſtance of the flame Q from the jet, is regulated by the pipe which holds the wick being a little removed from the centre of the braſs piece M, and of courſe revolving in a circle.

N the mahogany ſtand.

IX.

Deſcription of a Joint applied to Tubes uſed for conveying Steam under conſiderable Preſſure. W. N.

THE valuable communication of my correſpondent N. N. in the laſt number, has fixed my recollection upon ſeveral parts of my hydraulic and pneumatic apparatus, in which, though they may be called bagatelles as far as relates to the magnitude of the inventive powers required to make them, yet their utility may be ſuch as in many inſtances to prevent leſs eligible contrivances from being adopted. Recollection of ſome apparatus.

In the firſt place, I find among ſome braſs work the ingenious joint, Plate II. Fig. 2, lately contrived by himſelf, and before him by Mr. Webſter, but in this caſe certainly made many years ago by ſome unknown mechanic. My piece, which is for ſpouting fluids, is prevented from being driven aſunder by a wire that paſſes through its axis, and has a ſmall nut and waſher faced with leather. Early invention of the joint for pneumatic apparatus.

* If the bottom is made flat inſtead of ſpherical, the action of the flame will then be greater.

The

Another joint for strong steam.

The apparatus of which I have given a drawing and section in Plate VII. was made for conveying steam from a boiler to the steam engine, and for many other philosophical purposes, in which this agent is very useful, though hitherto not much applied to objects of this nature. As I have found it very commodious, I should not be disposed to make any remarks on the contrivance itself, if its resemblance to so many things that I have seen, both in organization and effect, did not make it proper to say, that I should have let it pass unpublished (among the many arrangements, whether old or new, which every practical mechanic finds himself induced to adopt in his operations) had not the consideration of utility made me suppose it might prove acceptable.

Delineation. A wide conical cock with joints in the tubes, pressed together with a screw and a dish spring.

In Fig. 1, 2, and 3, the same letters of the alphabet denote the same things; Fig 1 exhibiting a view, Fig. 2 a section, and Fig. 3 the plan of some of the parts. The letter *e* shews part of an upright tube conveying steam from the boiler into the body of the cock *c* by its hollow cone *m*, which is open beneath and closed above, and has a side aperture *n* communicating, when duly placed, with the pipe *h i*. The cone is pressed into its place by the cap *b* screwed on the body of the cock *c* at *r*. The cap does not press immediately upon the upper surface or shoulder of the cone, because the motion would in that case be hard and uncertain; but it acts by the intervention of a spring *o* in the shape of a dish or slightly concave place, which keeps it to its bearing with any required degree of pressure accordingly as the cap is more or less screwed down, and always with a most smooth and pleasant action without the least jerk or obstacle. The cock is turned by the wooden handle or lever *a*, and is very secure on account of the large surface of the convex and concave cones which remains imperforated.

The pipe between *h* and *i* is connected by a joint, in which *g* is a loose cap milled on the outside, and having a concave screw on its inner surface. It is screwed upon the fixed piece *f*, which is turned to fit it: In this last piece is a cylindric concavity surrounding the orifice of the pipe *h* (see *f*, Fig. 3), into which the piece *p*, at the extremity of the pipe *i*, loosely fits. Upon the circular face of *i* are turned three or four grooves, and the like on the correspondent face of *f*. These are of use to secure a small piece of tallowed linen cloth put between them

them when applied together. Laſtly, there is a diſh ſpring *o* between *p* and *g*. When *g* (which ſlides looſely upon the pipe *i*) is ſcrewed upon *f*, it preſſes *p* by the intervention of the ſtiff diſh ſpring againſt the interior of *f*, and makes a ſecure junction; but the pipe *i*, repreſented in Fig. 1 as pendant, or in a vertical poſition, may be moved round in the ſame plane to any elevation or depreſſion whatever.

The cap *d* being ſcrewed on the body of the cock at *t*, conſtitutes a joint of the ſame kind, by means of which the pipe *i* may be moved into any horizontal poſition: Conſequently, by theſe two motions, that pipe may be placed in every poſſible ſituation.

If we ſuppoſe the pipe *i* that proceeds immediately from the cock, to be of conſiderable length, as for convenience it is required to be, its poſſible changes of poſition will not be ſufficient to adapt it to the uſual operative purpoſes. It may therefore, in ſeveral caſes, be convenient to apply two other ſimilar joints *k* and *l* at right angles with each other, by means of which it will be immediately ſeen, that the exterior pipe *w* may be placed in any required direction, not merely with regard to a point immediately above the boiler, but with regard to another moveable point at an equal diſtance from the fire.

I ſhall conclude this paper by obſerving, that the bended waſher or diſh ſpring is uſed in the vice and ſome other mechanical tools, but that it far from being as much uſed as it ought to be. For there is ſcarcely any action of pinching, preſſing, or binding, in which it would not be advantageous, and would, in many caſes, prevent ſerious miſchief and inconveniences.

The diſh ſpring generally recommended.

X. *Memoir*

X.

Memoir on Achromatic Glasses adapted to the Measure of Angles, and the Advantages that may be derived from double Refraction for the precise Measurement of small Angles: by ALEXIS ROCHON, *Member of the National Institute, and Director of the Naval Observatory at Brest* *.

The position of places determined by angles.

WHEN we would determine the position of places, either on the earth or in the heavens, our conclusions are always founded on angles obtained by observation; and of course the certainty of our conclusions must depend on the precision with which we can measure these angles †. Euler was the first who attempted to correct the aberrations of refrangibility, by employing substances of different refractive powers. Maupertius procured Euler's object glass, consisting of glass and water, to be made at Paris: but this we now know could not succeed, from the ratios the refractions and dispersions of common glass and water bear to each other.

Euler said to have first attempted to correct the aberrations of refrangibility by a lens of glass and water;

and afterward Dollond by flint glass and crown glass;

In 1755, Mr. Dollond laid before the public his achromatic object glasses, composed of flint glass and crown glass. He says in his paper in the Philosophical Transactions, that he destroyed the aberrations of refrangibility with tolerable ease; but to destroy that of sphericity at the same time, was an object much more difficult to surmount. For this Dollond obtained a patent, the exclusive privilege of which was attacked by Mr. Watkins of Charing Cross, on the ground that the discovery had been made long before by a gentleman of the name of Hall, who however had not rendered it public. It is said that Mr. Hall gave orders for his lenses of flint glass to an optician in one part of London, and for those of crown glass to another in a distant part, that his secret might not be discovered; and that both of them happened to employ the same workman to make them, who suspecting some mystery, as he knew

an invention which he is said to have stolen from Hall.

* Abridged from the Journal de Physique, Fructidor, year 9.—C.

† The history of the invention of achromatic glasses is given very erroneously in this and most foreign memoirs. I purpose soon to give a commentary on this subject.—N.

knew they were for the same person, shewed them to Dollond, for whom likewise he worked, and who thus learned what he afterwards published as his own discovery.

Be it as it may, I shall with difficulty be persuaded, that Euler was not the first who thought of correcting the aberrations of refrangibility, as Newton invented the means of correcting the aberrations of sphericity, by composing an object glass of glass and water. Was it not the illustrious Euler, who, reflecting on the structure of the eye, suspected this organ was composed of different mediums for the purpose of destroying the confusion produced by the decomposition of light when it traverses a single medium? And did he not publish in the Memoirs of Berlin and of Petersburg, that sublime idea, which accorded with the system of perfection he ascribed to all the works of the Supreme? It is true, the inutility of this correction of the aberrations for the sphere of distinct vision, from the shortness of the form of the eye, may be objected to Euler's hypothesis: but this makes nothing against his claim to the discovery, to which he was led by it. Neither had this happy idea that success at the time, which might have been hoped from it; partly because the theory of this great geometrician was founded on laws of refraction purely hypothetical; partly because it was repugnant to the assertion of Newton, that, when light traverses several mediums of different natures, so that the emergent rays are parallel to the incident, the light is not decomposed. It is well known, that the learned Klingenstiern, in 1755, expressed his doubts of the laws of refraction advanced by Newton: and that these doubts were confirmed in 1759, by Dollond's experiments on crown and flint glass, which first exhibited to the learned colourless prisms.

But Euler the first discoverer of the principle, to which he was led from reflecting on the structure of the eye.

Though the correction he ascribed to the structure of the eye not necessary in this organ.

His idea neglected, because his theory was hypothetical, and contradictory to an assertion of Newton's.

Klingenstiern expressed his doubts of Newton's laws of refraction; and Dollond confirmed these doubts.

It was in the beginning of 1774, that I read to the academy a memoir, in which I proposed to improve achromatic object glasses by the interposition of a fluid between the two lenses of crown and flint glass which compose them. Borda, le Gentil, and Cassini, were appointed a committee to repeat my experiments. They observe in their report, that, "in an object glass of three lenses, if there be a difference of a thousandth part of a line between the curvature of the centre and that of the edges of each surface, a sensible imperfection in the image of the object will be the consequence. But the mere heat of the workman's hand, in giving the glass the finishing polish, is capable of dilating

Rochon proposes to improve Dollond's compounded object glass, by introducing a fluid between the pieces.

Report of Borda and others on this subject.

lating it sensibly, so that the difficulty of not producing some inequalities of curvature in large glasses must be very great. Perhaps it is impossible to avoid this imperfection, but it may be corrected: and our colleague affirms, that the effect of the imperfections of the four interior surfaces of the three glasses may be diminished considerably, by the introduction of a diaphanous fluid between them. Experiments alone can decide the fact. We took therefore an achromatic telescope three feet long, the aperture of which was about three inches. The two lenses composing the object glass, being about half an inch distant from each other, we introduced between them a thin Bohemian glass not wrought. In this state it is obvious the telescope must be very bad; and accordingly, on directing it to some writing, we were obliged to bring this within eleven yards and half of it, to distinguish the letters. Having then poured pure water between the object glasses, so as to fill the intervals between them, we found the letters not more difficult to be distinguished at the distance of sixty-two yards. We might have pursued our experiments further, but this was sufficient to establish the principle, which was all we had in view."

Their experiments on it, which are in favour of the principle.

At this time I was ordered to Brest, which put a stop to my pursuit. In my memoir I had advanced, that the confusion arising from the irregularity of the glass of a telescope, was to the confusion which takes place when the glass is immersed in a fluid, as the refraction of the glass is to the difference of refraction between the glass and the fluid: consequently, if the fluid have the same refractive power as the glass, no alteration in the distinctness of the object will be perceptible; but if the glass be immersed in water, the confusion will be about a seventh only of what would have taken place, had the object been seen directly through the irregular glass. This calculation, it is true, supposes the glass to be in perfect contact with the fluid; but there are obviously many causes by which this contact may be influenced. It may have been owing to some such cause, that oil, the refractive power of which differs less from that of glass, was found by experiment not to answer so well as water, for writing, which was legible at the distance of 130 feet when water was employed, could be read only at eighty-eight feet when oil was interposed between the glasses *.

To make a perfect object glass, the refractive power of the interposed fluid should be the same with that of the glass.

Water reduces the confusion to one seventh;

but oil, though its power is nearer that of glass, does not reduce it so much.

* On this subject, see Philos. Journal, 4to, III. 000.

By the Regifters of the Academy of Sciences, January the 6th, 1788, it appears, that Grateloup adopted the fame method. He did not make ufe of a fluid interpofed between the glaffes, however, but of maftic. This fubftance had long been in ufe among jewellers, for cementing together ftones, which it did in fuch a way, that it was fcarcely poffible to diftinguifh the two fo united from a fingle ftone; and hence Grateloup conceived, it would render his object glaffes as it were one folid piece, without being liable to evaporation like a fluid, while it corrected the errors of their interior furfaces. Glaffes cemented together with maftic, however, do not anfwer, at leaft for fea voyages, as change of temperature, and the fea air, affect the maftic very much. On this account I have preferred refin, and even turpentine the moft fluid and tranfparent I could get. Chemifts perhaps may find fubftances ftill preferable to thefe, and it is an object certainly worthy their attention. Dr. Blair tried a great number of folutions of metals and femimetals, and he fays, that certain falts, particularly fal ammoniac diffolved in water, give it a confiderable power of difperfion. The oxigenated muriatic acid, too, poffeffes this quality in a great degree; but butter of antimony has a ftill greater effect, for one prifm of this is equal to three of crown glafs of fimilar dimenfions. Dr. Blair conftructed an object glafs with crown glafs and butter of antimony, but he obferved, that it occafioned irradiations, which led him to prefer oil of turpentine or other effential oils *.

Grateloup ufed maftic between the glaffes.

But change of temperature, and the fea air, injure maftic.

Refin therefore preferable, or turpentine.

Dr. Blair tried various fubftances; fome falts, and oxigenated muriatic acid, had a confiderable power of difperfion; and muriate of antimony had ftill more; but this occafioned irradiations, whence he preferred effential oils.

The ufe of achromatic glaffes applied to graduate circles or fegments of circles for meafuring angles, and the defects to which inftruments of this kind are liable, may be paffed over as not to the prefent purpofe, which is the beft mode of meafuring very fmall angles.

Buffon, who had paid fome attention to the formation of rock cryftals, finding no indication of a double refraction in the experiments which I made on that of Madagafcar, in 1770, thought this tranfparent quarter to be of a different nature from the other cryftals; but before he made up his mind on the fubject, he defired me to examine it afrefh with a view to this property.

Buffon fufpected the double refraction of the rock cryftal of Madagafcar might have been overlooked.

* An ample abridgement of Dr. Blair's paper is given, Phil. Journal, 4to, I. 1.

Beccaria published a curious work on the refraction of crystals.

It may not be improper here to give a brief abstract of a very curious work, which Beccaria published in Italian about this time, as I learned from it the direction in which it was necessary to cut my prisms, to be certain of obtaining the effects I sought. Huygens, says Beccaria, who treats very fully on the double refraction of Iceland crystal, adds, that rock crystal possesses the same property, but in a less sensible degree. I saw that the refraction which Huygens calls irregular, bears a constant relation in the Iceland crystal to its internal structure; for the effect of this refraction is to divert the rays in the direction of the saliant angles, which are equal in the whole piece, and in each of its component parts. From inspecting the figure of rock crystal, I inferred its internal structure. In fact, I considered it as similar to artificial crystallizations. formed by the aggretation of little laminæ parallel to the faces of the crystals. Suspecting Huygens of inaccuracy in asserting, that prisms of rock crystal had always a double refraction, whatever their sections were, I cut a piece of rock crystal, of a very irregular figure, in the direction of its axis, and so as o divide two of its opposite faces into two equal parts. I cut another likewise in the direction of its axis, but so that the section passed through the summits of two opposite angles. A third afforded me triangular prisms, one of the faces of which was one of the faces of the crystal itself, while each of the other two faces terminated on one side at one of the contiguous angles, and on the other at the axis of the crystal. A fourth was divided into equilateral prisms, two of the faces of which were equally inclined to the axis, and the third was parallel to a plane continued through the axis. From the observations made with these prisms, I obtained a very simple law, of which Newton says nothing, and the contrary of which is advanced by Huygens. 1. That the double refraction does not subsist in all the different prisms that may be cut from rock crystal, 2. That the ray of light which traverses rock crystal in a place perpendicular to the axis undergoes two refractions, is divided into two, and gives two images, if not completely yet sensibly distinct. 3. That this distinctness of the two images diminishes in proportion as the direction of the ray approaches to that of the axis of the crystal. 4. That the double refraction and dispersion of the two images cease entirely, when the direction of the ray is parallel to the axis, or nearly parallel to it.

Huygens mistaken in calling the refraction irregular; as it uniformly depends on the internal structure of the crystal.

Beccaria cut rock crystal in different directions; whence he obtained certain laws of their refraction.

it. Hence it follows, that in cutting a lens of rock cryſtal for optical purpoſes, we muſt take pieces parallel to the baſe of the cryſtal, ſo that the axis of the lens coincides with that of the cryſtal, or is at leaſt parallel to it. Thus far Beccaria.

The refraction of the cryſtal is ſingle in the direction of its axis.

When we would apply to practice this properly in rock cryſtal of doubling the images of objects, the colours that ariſe from the priſmatic figure neceſſary to be given to it muſt be corrected by a glaſs priſm, or the images will not appear well defined. Theſe two priſms united together may be called a *double refracting achromatic medium*, but in this ſtate it can ſerve only to meaſure the ſmall angle given by the double refraction: yet it is eaſy to conceive, that on applying two double refracting mediums one on the other, we may vary the effect of the double refraction at pleaſure by a circular movement, as in the inſtrument for meaſuring colours which I laid before the Academy. Thus this double refraction, which has been deemed detrimental to the conſtruction of optical inſtruments, is in fact advantageous for the menſuration of ſmall angles, as I convinced the Academy, February the 25th, 1777, by an inſtrument conſtructed on this principle, which gave the meaſure of ſmall angles with a degree of preciſion, for which we could ſcarcely have hoped. This inſtrument, however, had the inconvenience of giving four images of one object, which occaſioned a conſiderable loſs of light; and the leſs light we have, the leſs the accuracy with which the point of contact can be obſerved.

A priſm of rock cryſtal muſt be corrected by one of glaſs:

the two forming a *double refracting achromatic medium.*

Inſtrument for meaſuring ſmall angles by means of two of theſe mediums.

Its defect.

Endeavouring to remedy this imperfection, I ſoon diſcovered a more ſimple conſtruction for the inſtrument, which I laid before the Academy the ſame year. In this only one double refracting achromatic medium is required, which is made to move along the inſide of any teleſcope in the axis of the object glaſs. The value of the double refraction is firſt to be determined by experiment, as in the former micrometer, the achromatic medium being placed againſt the object glaſs. It is then to be moved from the object glaſs toward the eye glaſs. The angle of the double refraction will now be what it was in the former ſituation, but the ſeparation of the images will be in the ratio that the diſtance of the refractive medium from the focus bears to the focal diſtance. Suppoſe for example the double refraction to be twenty-one minutes, and the focal diſtance of the object glaſs to be three metres: if the

This remedied by uſing one medium, made moveable along the axis of the teleſcope.

achromatic medium be brought nearer by two thirds of this diſtance, the double refraction will occaſion only a ſeparation of ſeven minutes between the images. Thus if we would meaſure the diameter of an object, we muſt move the achromatic medium toward the focus, till the two images of the object are ſeen preciſely in contact; when, having the angle of double refraction given, by previous experiment, the focal diſtance of the object glaſs, and the diſtance of the achromatic medium from the focus, the diameter will be given by the rule of proportion. It is obvious, that great care muſt be taken in determining the angle of double refraction at firſt with preciſion, as on this will depend the accuracy of all ſubſequent meaſures with the inſtrument.

Mode of uſing this micrometer.

Having thus improved the conſtruction of this micrometer, I found that the glaſs priſms, intended to correct the diſperſion of the priſm of rock cryſtal, left a refraction more and more perceptible in proportion as the double refracting medium was carried farther from the eye glaſs. I then availed myſelf of Beccaria's diſcovery, and cut my two priſms ſo that the firſt was in the direction of the double refraction, and the ſecond in that in which the double refraction is not perceptible: by theſe means I had a double refracting medium abſolutely exempt from colours and refractions. I did not ſtop here, for I was deſirous of extending the effect of the double refractions ſo as to meaſure the diameter of the ſun; and I accompliſhed this, which appeared to exceed the known power of the double refraction of rock cryſtal, which does not go beyond twenty minutes, when it is cut in the moſt advantageous priſmatic ſhape. For this I employed two equal priſms, cut in the direction moſt ſuitable to my purpoſe; and on placing them in oppoſite directions I found, that the double refraction was not perceptible; but on reverſing their directions the double refraction of each priſm was nearly doubled, ſo that I obtained two images ſeparated by an interval of forty minutes.

The inſtrument farther improved by uſing two priſms of rock cryſtal cut in oppoſite directions.

The effect of the double refraction doubled,

by means of two equal priſms.

I ought not to omit, that in this new conſtruction there are difficulties of execution not eaſy to ſurmount, which may have been one reaſon why theſe inſtruments, ſo uſeful to navigators and in certain very nice aſtronomical obſervations, have not been adopted. This induced me at length to adopt Euler's method. In the conſtruction of achromatic object glaſſes I found I could increaſe or diminiſh the absolute effect of the double

This inſtrument difficult to execute.

Hence the author was led to avail himſelf of Euler's method.

Effect of the

double refraction within certain limits, by means of the interval between the glasses of differing refracting powers: the separation of the images at the focus being so much the greater, as the interval is larger, when the flint glass is the first of the object glasses; and less, when it is the second.

double refraction increased by increasing the interval between the object glasses when the flint glass is first; the reverse when it is the second.

Conformably to these new principles I have had two telescopes with a double refracting medium constructed under my own inspection, which General Gantheaume will employ for determining the position of his ships, and to find whether he be approaching any he may meet with at sea *.

The uses of an instrument for measuring very small angles with precision are too well known for me to describe its advantages. The officers of the English navy are so fully aware of them, that they have used for some years Ramsden's eye glass micrometer, though this answers the end but imperfectly, because it does not give the measure of the angle, and because of the bad effect of the parallax produced by the decussation of the rays that enter the eye. This defect is more sensible in Ramsden's eye glass micrometer than in Bouguer's heliometer. The officers who have compared my instrument with Ramsden's, of which there were several on board the Spanish ships with our Brest fleet, agree that the celebrated English artist has very imperfectly accomplished the object he proposed; and Bouguer's heliometer could unquestionably be preferable for naval use, because it has a less sensible parallax, and gives the measure of small angles, so important for determining the distances of ships from their known dimensions.

Defects of Ramsden's eye glass micrometer.

Bouguer's heliometer preferable.

Table of the proportions which the magnitude of an object bears to its distance: calculated according to the rule, which is easily demonstrated, that, in every right angled triangle, the tangent is to the radius as the magnitude of an object is to its distance from the centre of the eye, when, under an angle known, the distance forms a right angled triangle with the object.

* The General has made an advantageous report of this instrument, in his account of the chace of the Swiftsure, which he captured. This instrument is so difficult to execute, that I know only one person, Citizen Narci, who is capable of giving rock crystal the prismatic form in the proper direction for obtaining the double refractions necessary to the goodness of the micrometer. He has made several, which have given me perfect satisfaction.

only one person yet found capable of making this micrometer.

EXAMPLE.

EXAMPLE.

General formula for calculating distances.

Suppose the angle be measured under which we perceive an object of the magnitude of one toise, placed vertically so as to represent the tangent, the radius of which is the distance from the eye; if this angle be of 30′ we shall have the following proportion: the tangent of 30′ is to the radius, as one toise is to the distance. By the tables of logarithms we shall have this distance by subtracting from the logarithm of the cosine the logarithm of the tangent of 30′, which is 7.940858: the logarithm resulting from this subtraction is 2.059142; and the number answering to this logarithm is 114.6 nearly; consequently the distance from the eye to the object, the magnitude of which is supposed to be one toise, is 114 toises six tenths; but in the use of the instrument fractions may be neglected.

It is proper to observe, that in measuring small angles the triangle need not be perfectly rectangular: the object may sensibly vary from the perpendicular, without a perceptible difference in the angle.

QUESTION I.

Mode of determining by the micrometer the distance of an object of known magnitude.

The magnitude of an object being known, to determine its distance.

For this purpose the two images of the object produced by the prisms are to be brought into contact, by moving the index along the axis that carries them. This rule has two divisions, one shewing the minutes and seconds, the other the proportion between the diameter of the object and the distance. Suppose the object I look at is a man: the mean height of a man with his hat on may be estimated at six feet. Having disposed the prisms and telescope so that the two images are in one vertical line, I move the index till the feet of the upper image come into contact with the summit of the lower: if now the index point at the number 344, I conclude that the distance of the man from my eye is 344 times six feet, or 344 toises. The second division will shew, that the angle under which I see the man is 10′. By this method the measure of a distance may unquestionably be obtained with great precision, but there are many cases, in which this great precision is not necessary.

Ships,

Ships, buildings in which certain rules little liable to variation are obferved, and the dimenfions of different animals, may ferve to give this diftance. By means of fuch objects I have often determined inacceffible diftances with truly furprifing celerity, by means of a portable telefcope with prifms, which requires no fupport for taking angles. When greater accuracy is required, objects of which the diameter is well afcertained muft be employed.

Croffes of known dimenfions on light-houfes would be of great utility to navigation.

If lighthoufes had on them a crofs of fixed and known dimenfions, it would thus be of great fervice to navigators: for by the angle of the perpendicular piece, given by this micrometer, the diftance of the fhip from the lighthoufe would be afcertained; and by that of the horizontal piece would be known the pofition of the fhip with refpect to the coaft.

QUESTION II.

The diftance of an object being given, to determine its magnitude.

The magnitude of an object determined from the diftance known.

This is precifely the reverfe of the preceding.

QUESTION III.

The magnitude and diftance of an object being unknown, to determine both.

Method of determining the magnitude and diftance, where both are unknown.

Suppofe the object makes an angle of 40′ with my eye; as this angle will increafe in regular proportion as the diftance is diminifhed, I approach the object till the angle is 41′, a difference which will be fufficient in ordinary cafes. If I find the diftance between the two ftations, on meafuring it, to be a hundred toifes, I fhall obtain the whole diftance by multiplying this by the number of minutes of the larger angle, and dividing the product by the difference between the two angles, which will give in this inftance 4100 toifes. The diftance being thus obtained, the magnitude of the object will be found by dividing 4100 toifes by 83½, the number anfwering to 41′; which will give the magnitude forty-nine toifes feven inches nearly.

XI. *Obfervations*

XI.

Obſervations on the two lately diſcovered celeſtial Bodies. By WILLIAM HERSCHEL, *LL. D. F. R. S.*

The moving ſtar of Piazzi is very minute.

IN my early account of the moving ſtar diſcovered by Mr. Piazzi, I have already ſhewn that it is of a remarkably ſmall ſize, deviating much from that of all the primary planets *.

It was not my intention to reſt ſatisfied with an eſtimation of the diameter of this curious object, obtained by comparing it with the Georgian planet, and, having now been very ſucceſsful in the application of the lucid diſk micrometer, I ſhall relate the reſult of my inveſtigations.

Another diſcovered by Dr. Olbers.

But the very intereſting diſcovery of Dr. Olbers having introduced another moving ſtar to our knowledge, I have extended my reſearches to the magnitude, and phyſical conſtruction, of that alſo. Its very particular nature, which, from the obſervations I ſhall relate, appears to be rather cometary than planetary, will poſſibly throw alſo conſiderable light upon the circumſtances belonging to the other celeſtial body; and, by that means, enable us to form ſome judgment of the nature of both the two laſt diſcovered phenomena.

As the meaſures I have taken will oblige me to give a reſult which muſt appear extraordinary, it will be highly neceſſary to be particular in the circumſtances of theſe meaſures, and to mention the condition and powers of the teleſcopes that were uſed to obtain them.

Magnitude of the new Stars.

Obſervations on *Ceres* compared with a lucid diſk.

7-feet reflector.

April 1, 1802. Having placed a lucid diſk at a conſiderable diſtance from the eye, but ſo that I might view it with perfect diſtinctneſs, I threw the image of Mr. Piazzi's ſtar, ſeen in a 7-feet reflector, very near it, in order to have the projected picture of the ſtar and the lucid diſk ſide by ſide, that I might aſcertain their comparative magnitudes. I ſoon perceived that the length of my garden would not allow me to remove the diſk-micrometer, which muſt be placed at right

* By comparing its apparent diſk with that of the Georgian planet, it was eſtimated, that the real diameter of this new ſtar could not amount to three-eighths of that of our moon.

angles

angles to the telefcope, far enough to make it appear no larger than the ftar; and, not having difks of a lefs diameter prepared, I placed the fmalleft I had as far from me as the fituation of the ftar would allow. Then, bringing its image again by the fide of the difk, and viewing, at the fame time, with one eye the magnified ftar, while the other eye faw the lucid difk, I perceived that Ceres, which is the name the difcoverer has given to the ftar, was hardly more than one third of the diameter of the difk, and certainly lefs than one half of it.

This being repeated, and always appearing the fame, we fhall not under-rate the fize of the ftar, by admitting its diameter to have been 45 hundredths of the lucid difk.

The power of the telefcope, very precifely afcertained by terreftrial geometrical meafures properly reduced to the focus of the mirror on the ftars, was 370,42. The diftance of the lucid difk from the eye, was 2131 inches; and its diameter 3,4 inches. Hence we compute, that the difk was feen under an angle of 5′ 29″,09; and Ceres, when magnified 370 times, appearing, as we have fhewn, 45 hundredths of that magnitude, its real diameter could not exceed 0″,40. Had this diameter amounted to as much as was formerly eftimated, the power of 370 would have made it appear of 6′ 10″, which is more than the whole lucid difk. Power 370,42. Diameter of Ceres 0″,40.

This extraordinary refult raifed in me a fufpicion, that the power 370 of a 7-feet telefcope, and its aperture of 6,3 inches, might not be fufficient to fhew the planet's feeble light properly. I therefore adapted my 10-feet inftrument to obfervations with lucid difks; which require a different arrangement of the head of the telefcope and finder: I alfo made fome fmall tranfparencies, to reprefent the object I intended to meafure.

April 21. The night being pretty clear, though perhaps not quite fo proper for delicate vifion as I could have wifhed, I directed my 10-feet reflector, with a magnifying power of 516,54, alfo afcertained by geometrical terreftrial meafures reduced to the focus of the inftrument on celeftial objects, to Mr. Piazzi's ftar, and compared it with a lucid difk, placed at 1486 inches from the eye, and of 1,4 inch in diameter. I varied the diftance of the lucid difk many times; and fixed at laft on the above-mentioned one, as the beft I could find. There was, 2. Obfervation repeated on Ceres with 10-feet reflector. Power 516,54.

however,

Hazinefs. however, a hazinefs about the ftar, which refembled a faint coma; and this, it may be fuppofed, muft render the meafure lefs fatisfactory than it would otherwife have been.

Diameter of Ceres 0″,38. From thefe data we compute, that the difk appeared to the natural eye under an angle of 3′ 14″,33; while Ceres, when magnified $516\frac{1}{2}$ times, was feen by the other eye of an equal magnitude; and that confequently its real diameter, by meafurement, was only 0″,38.

3. Obfervation repeated on Ceres; 10-feet reflector, new fmall mirror; April 22. 11^h 38′, fidereal time. I ufed now a more perfect fmall mirror; the former one having been injured by long continued folar obfervations. This gave me the apparent diameters of the ftars uncommonly well defined; to which, perhaps, the very favourable and undifturbed clearnefs of the atmofphere might contribute confiderably.

power 881,51; With a magnifying power of 881,51, properly afcertained, like thofe which have been mentioned before, I viewed Dr. Olbers's ftar, and compared it with a lucid difk of 1,4 inch in diameter, placed at 1514 inches from the eye, meafured, like bad obf. the reft of the diftances, with long deal rods. The ftar appeared to me fo ill defined, that, afcribing it to the eye-glafs, I thought it not adviseable to compare the object, as it then Power changed to $516\frac{1}{2}$. appeared, with a well defined lucid difk. Exchanging the glafs for that which gives the telefcope a magnifying power of *Pallas*, $516\frac{1}{2}$, I found Pallas, as the difcoverer wifhes to have it called, better defined; and faw, when brought together, that it was confiderably lefs in diameter than the lucid difk.

In order to produce an equality, I removed the difk to 1942 inches; and ftill found Pallas confiderably lefs than the difk.

fmaller than Ceres. Before I changed the diftance again, I wifhed to afcertain whether Ceres or Pallas would appear under the largeft angle, efpecially as the air was now more pure than laft night. On comparing the diameter of Ceres with that of the lucid difk, I found it certainly lefs than the difk. By proper attention, and continued examination, for at leaft an hour, I judged it to be nearly $\frac{3}{4}$ of the lucid difk.

Then, if we calculate as before, it appears by this obfervation, in which there is great reafon to place confidence, that the angle under which this ftar appeared, was only 0″.22. For, a lucid difk of 1,4 inch diameter, at the diftance of 1942

inches,

inches, would be feen under an angle of 2′ 28″,7; three quarters of which are 1′ 51″,52. This quantity, divided by the power 516,54, gives 0″,2159, or, as we have given it abridged, 0″,22. Excellent refult of Ceres, diam. 0″,22.

13^h 7′. I removed the micrometer to the greateft convenient diftance, namely, 2136 inches, and compared Dr. Olbers's ftar, which, on account of its great altitude, I faw now in high perfection, with the lucid difk. It was, even at this diftance, lefs than the diameter of the difk, in the proportion of 2 to 3.

When, by long continued attention, the appearance of Pallas was reduced to its fmalleft fize, I judged it to bear no greater proportion to the diameter of the lucid difk of the micrometer, than as 1 to 2.

In confequence of thefe meafures, it appears that the diameter of Pallas, according to the firft of them, is 0″,17; and, according to the laft, where the greateft poffible diftinctnefs was obtained, only 0″,13. Diam. of Pallas 0″,13.

If it fhould appear almoft incredible that thefe curious objects could give fo fmall an image, had they been fo much magnified as has been reported, I can fay, that curiofity led me to throw the picture of Jupiter, given by the fame telefcope and magnifying power, on a wall at the diftance of 1318 inches, of which it covered a fpace that meafured 12 feet 11 inches. I do not mention this as a meafure of Jupiter, for the wall was not perfectly at right angles to the telefcope, on which account the projected image would be a little larger than it fhould have been, nor was I very attentive to other neceffary minute circumftances, which would be required for an accurate meafure; but we fee at once, from the fize of this picture, that the power of the telefcope exerted itfelf to the full of what has been ftated. Proof by the image of Jupiter that the power of the telefcope was truly as here ftated.

As we generally can judge beft of comparative magnitudes, when the meafures are, as it were, brought home to us; it will not be amifs to reduce them to miles. This, however, cannot be done with great precifion, till we are more perfectly acquainted with the elements of the orbits of thefe ftars. But, for our prefent purpofe, it will be fufficiently accurate, if we admit their mean diftances from the fun, as the moft recent information at prefent ftates them; for Ceres 2,6024; and for Pallas 2,8. The geocentric longitudes and north latitudes, at the time of Rough computation of the diameters of thefe bodies in miles:

of observation, were, for Ceres, about ♍ 20° 4′, 15° 20′; and for Pallas, ♍ 23° 40′, 17° 30′. With these data, I have calculated the distances of the stars from the earth at the time of observation, partly by the usual method, and, where the elements were wanting, by a graphical process, which is sufficiently accurate for our purpose. My computed distances were 1,634 for Ceres, and 1,8333 for Pallas; and, by them we find, that the diameter of Ceres, at the mean distance of the earth from the sun, would subtend an angle of 0″,35127; and that, consequently, its real diameter is 161,6 miles.

That of Ceres proves to be 161,6 miles; and that of Pallas 147 or 110½ miles.

It also follows, that Pallas would be seen, at the same distance from the sun, under an angle of 0″,3199; and that its real diameter, if the largest measure be taken, is 147 miles; but, if we take the most distinct observation, which gives the smallest measure, the angle under which it would be seen from the sun, will be only 0″,2399; and its diameter, no more than 110½ miles.

Of Satellites.

Examination for discovering Satellites.

After what has just now been shewn, with regard to the size of these new stars, there can be no great reason to expect that they should have any satellites. The little quantity of matter they contain, would hardly be adequate to the retention of a secondary body; but, as I have made many observations with a view to ascertain this point, it will not be amiss to relate them.

None were ascertained.

Feb. 25. 20-feet reflector. There is no small star near Ceres, that could be supposed to be a satellite.

Feb. 28. There is no small star within 3 or 4 minutes of Ceres, that might be taken for a satellite.

March 4. 9^h. 45′. sidereal time. A very small star, south-preceding Ceres, may be a satellite. See Plate V. Fig. 1. where C is Ceres, S the supposed satellite, *a b c d e f*, are delineation stars, *c* and *d* are very small. S makes nearly a right angle with them; *e* is larger than either *c* or *d*. There is an extremely faint star *f*, between *e* and *d*.

14^h. 16′. Ceres has left the supposed satellite behind.

March 5. There are two very small stars, which may be satellites; see Fig. 2. where they are marked, 1st S, 2d S. The rest, as before, are delineation stars.

March 6. The two supposed satellites of last night remain in their situation, Ceres having left them far behind.

10h

10^h. 16′. There is a very ſmall ſtar, like a ſatellite, about 75° ſouth-following Ceres. See Fig. 3. It is in a line from C to *b* of laſt night.

11^h. 20′. Ceres has advanced in its orbit; but has left the ſuppoſed ſatellite behind.

March 30. 9^h. 35′. A ſuppoſed 1ſt ſatellite is directly following Ceres: it is extremely faint. A 2d ſuppoſed ſatellite is north-following. See Fig. 4. The ſuppoſed ſatellites are ſo ſmall, that, with a 20-feet teleſcope, they require a power of 300 to be ſeen; and the planet ſhould be hidden behind a thick wire, placed a little out of the middle of the field of view, which muſt be left open to look for the ſuppoſed ſatellites.

12^h 17′. Ceres has changed its place, and left both the ſuppoſed ſatellites behind.

March 31. 9^h 20′. There is a very ſmall ſtar, on the north-preceding ſide of Ceres, which may be a ſatellite.

11^h 50′. Ceres has moved forwards in its path; but the ſuppoſed ſatellite remains in its former ſituation. The neareſt ſtar is 20″ of time from Ceres; ſo that, within a circle of 40″ of time, there certainly is no ſatellite that can be ſeen with the ſpace-penetrating power of this inſtrument.

It is evident, that when the motion of a celeſtial body is ſo conſiderable, we need never be long in doubt whether a ſmall ſtar be a ſatellite belonging to it, ſince a few hours muſt decide it.

May 1. 12^h 51′. I viewed Pallas with the 20-feet reflector, power 300; there was no ſtar within 3′, that could be taken for a ſatellite.

Of the Colour of the new Stars.

Feb. 13. The colour of Ceres is ruddy, but not very deep. Ceres is of a ruddy colour; Pallas duſky white.
April 21. Ceres is much more ruddy than Pallas.
April 22. Pallas is of a duſky whitiſh colour.

Of the Appearances of the new Stars, with regard to a Diſk.

Feb. 7. Ceres, with a magnifying power of 516½, ſhews an ill defined planetary diſk, hardly to be diſtinguiſhed from the ſurrounding hazineſs. The diſk of Ceres is ill-defined;

Feb. 13. Ceres has a viſible diſk.

April

April 22. In viewing Pallas, I cannot, with the utmoſt attention, and under the moſt favourable preſent circumſtances, perceive any ſharp termination which might denote a diſk; it is rather what I would call a nucleus.

Pallas is not a diſk but a nucleus.

April 28. In the finder, Pallas is leſs than Ceres. It is alſo rather leſs than when I firſt ſaw it.

Of the Appearances of the new Stars, with regard to an Atmoſphere, or Coma.

Ceres has moſt frequently a coma (perhaps effected by the inſtrument, or more probably by the atmoſphere.)

April 21. I viewed Ceres for nearly an hour together. There was a hazineſs about it, reſembling a faint coma, which was, however, eaſily to be diſtinguiſhed from the body.

April 22. I ſee the diſk of Ceres better defined, and ſmaller than I did laſt night. There does not ſeem to be any coma; and I am inclined to aſcribe the appearance of laſt night to a deception, as I now and then, with long attention, ſaw it without; at which times it was always beſt defined, and ſmalleſt.

April 28. Ceres is ſurrounded with a ſtrong hazineſs. Power 550.

With 516½, which is a better glaſs, the breadth of the coma beyond the diſk may amount to the extent of a diameter of the diſk, which is not very ſharply defined. Were the whole coma and ſtar taken together, they would be at leaſt three times as large as my meaſure of the ſtar. The coma is very denſe near the nucleus; but loſes itſelf pretty abruptly on the outſide, though a gradual diminution is ſtill very perceptible.

April 30. Ceres has a viſible, but very ſmall coma about it. This cannot be ſeen with low powers; as the whole of it together is not large enough, unleſs much magnified, to make up a viſible quantity.

May 1. The diameter of the coma of Ceres, is about 5 times as large as the diſk, or extends nearly 2 diameters beyond it.

13ʰ 19′. 20-feet reflector; power 477. The diſk of Ceres is much better defined than that of Pallas. The coma about it is conſiderable, but not quite ſo extended as that of Pallas.

May 2. 13ʰ 20′. Ceres is better defined than I have generally ſeen it. Its diſk is ſtrongly marked; and, when I ſee it beſt, the hazineſs about it hardly exceeds that of the ſtars of an equal ſize.

Memorandum.

Memorandum. This may be owing to a particular dispoſition of the atmoſphere, which ſhews all the ſtars without twinkling, but not quite ſo bright as they appear at other times. Jupiter likewiſe has an extremely faint ſcattered light about it, which extends to nearly 4 or 5 degrees in diameter.

April 22. Pallas, with a power of $881\frac{1}{2}$, appears to be very ill defined. The glaſs is not in fault; for, in the day time, I can read with it the ſmalleſt letters on a meſſage card, fixed up at a great diſtance. Pallas is ill-defined, nebulous or cometary.

$13_h\ 17'$. The appearance of Pallas is cometary; the diſk, if it has any, being ill defined. When I ſee it to the beſt advantage, it appears like a much compreſſed, extremely ſmall, but ill defined, planetary nebula.

April 28. Pallas is very ill defined: no determined diſk can be ſeen. The coma about it, or rather the coma itſelf, for no ſtar appears within it, would certainly meaſure, at firſt ſight, 4 or 5 times as much as it will do after it has been properly kept in view, in order to diſtinguiſh between the hazineſs which ſurrounds it, and that part which may be called the body.

May 1. Pallas has a very ill defined appearance; but the whole coma is compreſſed into a very ſmall compaſs.

$13^h\ 5'$. 20-feet reflector; power 477. I ſee Pallas well, and perceive a very ſmall diſk, with a coma of ſome extent about it, the whole diameter of which may amount to 6 or 7 times that of the diſk alone. Very ſmall diſk.

May 2. $13^h\ 0'$. 10-feet reflector. A ſtar of exactly the ſame ſize, in the finder, with Pallas, viewed with $516\frac{1}{2}$, has a different appearance. In the centre of it is a round lucid point, which is not viſible in Pallas. The evening is uncommonly calm and beautiful. I ſee Pallas better defined than I have ſeen it before. The coma is contracted into a very narrow compaſs; ſo that perhaps it is little more than the common aberration of light of every ſmall ſtar. See the memorandum to the obſervations of Ceres, May 2.

On the Nature of the new Stars.

From the account which we have now before us, a very important queſtion will ariſe, which is, What are theſe new ſtars, are they planets, or are they comets? And, before we Nature of the new ſtars.

can

can enter into a proper examination of the subject, it will be necessary to lay down some definition of the meaning we have hitherto affixed to the term planet. This cannot be difficult, since we have seven patterns to adjust our definition by. I should, for instance, say of planets,

Criterions of planets.

1. Magnitude.

1. They are celestial bodies, of a certain very considerable size.

2. Orbits nearly circular.

2. They move in not very excentric ellipses round the sun.

3. Nearly in the pl. ecliptic.

3. The planes of their orbits do not deviate many degrees from the plane of the earth's orbit.

4. Motion direct.

4. Their motion is direct.

5. Satellites or rings.

5. They may have satellites, or rings.

6. Atmosphere, not extensive.

6. They have an atmosphere of considerable extent, which however bears hardly any sensible proportion to their diameters.

7. Orbits considerably asunder.

7. Their orbits are at certain considerable distances from each other.

Comparison of the new stars in these respects.

Now, if we may judge of these new stars by our first criterion, which is their size, we certainly cannot class them in the list of planets: for, to conclude from the measures I have taken, Mercury, which is the smallest, if divided, would make up more than 135 thousand such bodies as that of Pallas, in bulk.

In the second article, their motion, they agree perhaps sufficiently well.

The third, which relates to the situation of their orbits, seems again to point out a considerable difference. The geocentric latitude of Pallas, at present, is not less than between 17 and 18 degrees; and that of Ceres between 15 and 16; whereas, that of the planets does not amount to one half of that quantity. If bodies of this kind were to be admitted into the order of planets, we should be obliged to give up the zodiac; for, by extending it to them, should a few more of these stars be discovered, still farther and farther deviating from the path of the earth, which is not unlikely, we might soon be obliged to convert the whole firmament into zodiac; that is to say, we should have none left.

In the fourth article, which points out the direction of the motion, these stars agree with the planets.

(To be continued in our next.)

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AND
THE ARTS.

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ARTICLE I.

Comparative Experiments and Obſervations on Myrtle Wax, Bees Wax, Spermaceti, Adipocire, and the Cryſtalline Matter of biliary Calculi. In a Letter from JOHN BOSTOCK, *M. D.*

To Mr. NICHOLSON.

Liverpool, Jan. 23, 1803.

SIR,

IN the courſe of laſt ſummer, I was requeſted by a friend, who had received from America a quantity of the peculiar vegetable matter called Myrtle Wax, to give him ſome information reſpecting its nature and properties, and the probable uſe to which it might be advantageouſly applied. For this purpoſe I examined ſeveral works on natural hiſtory and chemiſtry, but I could only meet with a general account of the method employed for obtaining it, with a vague deſcription of its phyſical properties. Reſpecting its chemical analyſis I could find nothing ſatisfactory; even the elaborate work of Fourcroy contains little preciſe information on this head *. I determined

Enquiry concerning the nature and uſes of myrtle wax.

Scarcely mentioned by Fourcroy.

* Fourcroy, Syſtême des Connoiſſances Chimiques, tom. vii. p. 339.

determined therefore to examine this point more accurately, and with this intention, the experiments of which I send you an abstract were performed. Some other subjects however having at that time occupied me, I had almost forgotten my analysis of the myrtle wax, until in looking over the 131st No. of the *Annales de Chimie*, I found that the subject had been treated in an ample manner by M. Cadet. This circumstance induced me to recur to my Papers, in order to compare my experiments with those of M. Cadet; when I found that though his memoir contains a very complete account of the natural history of the substance, yet that my chemical analysis was more minute. On this account I conceive, that if you have any intention of inserting in your Journal * a translation of M. Cadet's essay, my communication may prove a useful supplement.

But more fully by Cadet.

I am,

Your obedient servant,

JOHN BOSTOCK.

Description and Analysis of Myrtle Wax.

Obvious properties of myrtle wax.

The peculiar substance obtained from the Myrica Cerifera of Linnæus †, and which has obtained the name of myrtle wax, is a concrete body, of moderate hardness and consistence; it has in part the tenacity of bees wax, though without its unctuosity; along with this, it also possesses, in some degree, the brittleness of the resins. The colour of the myrtle wax is a pale green; the shades of the different pieces are however somewhat varied; in most of them, the green has a tendency to a dirty grey; in others, it is lighter, more transparent, and of a yellowish tinge. Its specific gravity is about 1,0150, water being 1,0000, and white bees wax, 9600. It is fused at a temperature of 109°; by sufficiently increasing the heat, it burns with a peculiarly clear, white flame, produces little smoke, and during the combustion emits an agreeable aromatic odor.

Habitudes with reagents.

The following are its habitudes with the different re-agents.

Water has no action.

1. Water has no action upon the myrtle wax, either when cold or at the boiling heat.

* The reader will find it in the present Number. N.

† Martyn's edition of Millar's Dictionary.

Alcohol,

2. Alcohol, at the ordinary temperature of the atmosphere, has no action upon it; but 100 parts, by weight, of this fluid, when boiling, dissolve about five parts of the wax. Nearly $\frac{4}{5}$ is deposited by the cooling of the alcohol; $\frac{1}{5}$ still remains suspended; this is slowly deposited in the course of a few days, or may be precipitated by the addition of water. This substance, when precipitated from the alcohol, is lighter coloured than in its original state, and approaches more to a grey tinge. Though the myrtle wax seems to be homogeneous in its texture and consistence, it is not totally soluble in alcohol: about $\frac{4}{5}$ only of the whole is acted upon by this fluid, even when boiling. The part which is insoluble in alcohol, when separated from the rest of the mass, exhibits a somewhat darker shade of green than before the experiment; the alcohol remains completely colourless.

Cold alcohol does not act upon it. Hot alcohol sparingly dissolves it in part only, and deposits this by cooling and repose.

3. Sulphuric ether, when at the common temperature of the atmosphere, dissolves the myrtle wax only in small quantity, but it acts upon it rapidly when boiling. On account of the tendency which the fluid has to evaporate, it is difficult to ascertain the exact proportion, but it seems to take up somewhat more than $\frac{1}{4}$ of its own weight. The greatest part of this is separated as the ether cools, and the rest may be precipitated by the addition of water. The wax, after it has been dissolved in ether, is nearly colourless, while the fluid assumes a beautiful green hue. If the wax be not too abundant, and the ether be suffered to evaporate slowly, the wax is deposited on the sides of the glass in a crystalline or lamellated form; in this state its texture approaches somewhat to that of spermaceti.

Cold S. Ether dissolves little; hot ether more rapidly and largely. The deposit by evaporation resembles spermaceti.

4. Rectified oil of turpentine at the temperature of the atmosphere softens the wax, but does not seem capable of dissolving it; when assisted by a moderate heat, 100 grains of the turpentine dissolve six grains of the myrtle wax. The turpentine acquires a light green tinge, part of the wax is separated as the fluid cools, while part remains permanently dissolved in it.

Ol. Turpentine acts but slightly.

5. When the myrtle wax is boiled with liquid caustic potash, the fluid becomes turbid, but after some time the greatest part of the wax rises to the surface, nearly without colour, in a flocculent form. A small quantity of it only remains dissolved

Pure potash in water renders it colourless by boiling, and forms a soap with a small part, which be-

ing decompofed by acid affords the wax nearly unchanged.

folved in the potafh; this may be precipitated from it by an acid. That part of the wax which rifes to the furface is converted into a faponaceous matter; it has loft its inflammability and fufibility, and forms an opake folution with water. From this folution it is precipitated by an acid in the form of white flakes, which when collected, will be found to refemble very nearly the wax before its union with the potafh.

Pure ammonia acts nearly as potafh, but more feebly.

6. Pure ammonia exhibits with the myrtle wax phenomena in many refpects, fimilar to thofe produced by the fixed alcalis. When its action is promoted by heat, an opake folution is produced; the wax is deprived of its colour; the greateft part of it feparates from the fluid, and is converted into a fubftance partly foluble in warm water, though lefs fo than that refulting from the action of potafh upon myrtle wax.

The three principal mineral acids act upon it; though not with any notable force.

7. The mineral acids exercife little action upon this fubftance at the ordinary temperature of the atmofphere; the fulphuric diffolves it fparingly, and acquires a brown tinge. With the affiftance of a moderate heat this acid diffolves about $\frac{1}{12}$ of its weight, and is converted into a thick, dark-brown mafs; by cooling it becomes nearly concrete, but no feparation of the wax takes place. The nitric and muriatic acids, even when heated, feem to poffefs little attraction for the myrtle wax. After the wax had been kept for fome time in a ftate of fufion in contact with the nitric acid, its green hue was converted into a pale yellow, but the acid remained colourlefs, nor did any part of the wax appear to be diffolved in it: the wax by long digeftion in muriatic acid became of a bright orange colour.

Deduction: that myrtle wax appears to be an oxigenated fixed oil.

Thefe experiments will enable us, at leaft with a confiderable degree of probability, to affign the place which the vegeable myrtle wax muft hold in a natural arrangement of chemical fubftances. Its inflammability, fufibility, its infolubility in water, and the action which takes place between it and the alcalis, point out its affinity to the fixed oils, while its texture and confiftence, and more particularly its habitudes with alcohol and ether, indicate a refemblance to the refins. We may therefore confider the myrtle wax as a fixed vegetable oil, rendered concrete by the addition of a quantity of oxigen; it feems to hold the fame relation to the fixed, that refins do to the effential oils of vegetables.

But

But though the myrtle wax be itſelf of vegetable origin, there are ſome animal ſubſtances which more nearly reſemble it in its chemical properties than any product of the vegetable kingdom. The principal of theſe is the wax elaborated by the bee, to which the peculiar ſubſtance now under conſideration bears a ſtrong reſemblance, both in its phyſical and chemical properties. Myrtle wax alſo in many particulars reſembles ſpermaceti; the ſubſtance called Adipocire, produced by the action of nitric acid upon the muſcular fibre; and the cryſtalline matter of biliary calculi. I ſhall in a very conciſe manner, inſtitute a compariſon between myrtle wax and each of theſe ſubſtances.

But it reſembles the wax of the bee; and alſo ſpermaceti; and the adipocire produced by nitric acid upon muſcular fibre; and the cryſtalline matter of biliary calculi:

Bees Wax.

This ſubſtance, in its phyſical properties, differs from myrtle wax in being more unctuous, and poſſeſſing a greater degree of tenacity: its colour and ſmell are alſo different. Bees wax is likewiſe conſiderably leſs fuſible; Dr. Pearſon * and Mr. Nicholſon † fix its melting point at 142°; whereas Mr. Fourcroy ‡ places it lower in the ſcale at 117°; on this ſubject the reſults of my experiments coincide exactly with thoſe of the Engliſh chemiſts. There is alſo a difference of opinion among chemiſts reſpecting the action which takes place between this ſubſtance and alcohol; Fourcroy §, Chaptal ||, and Nicholſon **, aſſert that it is inſoluble in this fluid, while Pearſon maintains the contrary; on this queſtion my experience agrees with that of Dr. Pearſon. The proportion of bees wax which the alcohol is capable of diſſolving, ſeems however to be ſomewhat leſs than that of the myrtle wax. As in the former caſe, the greateſt part of the wax ſeparates as the fluid cools; while the remainder may be precipitated by the addition of water. Bees wax is ſparingly diſſolved by boiling ether, leſs readily, and in conſiderably leſs proportion

Compariſon with bees wax. Myrtle wax is more unctuous; tenacious; different in colour and ſmell; more fuſible, more ſoluble in alcohol and in ether; more affected by potaſh, and nearly the ſame by ammonia.

* Pearſon's Obſervations and Experiments on White Lac. Phil. Tranſ. 1794.

† Nicholſon's Journal, Quarto, vol. i. p. 70.

‡ Fourcroy Syſtême des Connaiſſ. Chimiques, x. 343.

§ Ibid. and Thomſon's Fourcroy, Vol. iii. p. 387.

|| Chaptal's Elements, Vol. III. p. 164.

** (Nicholſon's Elements, p. 502.)

than

than the myrtle wax; this fluid, when heated, feems only to take up about $\frac{1}{20}$ of its weight of bees wax. Cauftic potafh exhibits the fame phenomena with bees wax as with the product of the myrica cerifera; it was converted into the faponaceous ftate, and became foluble in warm water. It appeared however that the action was lefs violent, and the change lefs complete than in the former cafe. Ammoniac, when boiling, readily forms with bees wax an emulfion, in fome refpects refembling that produced by the fame fubftance with the myrtle wax. As the mixture cools, the greateft part of the wax rifes to the furface in a flocculent form; it appears to have fo far contracted a union with the alcali as to have its texture and odour deftroyed, and its fufibility and inflammability diminifhed; yet it is little, if at all, foluble in water.

Spermaceti.

Spermaceti: Is more fufible than either of the preceding.

One property of this fubftance, which obvioufly diftinguifhes it from thofe already defcribed, is the cryftalline texture which it conftantly affumes. It is more fufible than either of the fubftances which we have examined; but refpecting the precife temperature at which it becomes liquid, there is a confiderable difference of opinion. Fourcroy * ftates it to be at the 98th degree, or a little lower †; Mr. Nicholfon ‡ fuppofes it to be at the 133d degree; I have found the melting point of fpermaceti to be uniformly 112°; there may perhaps be a real difference in the fpecimens that have been employed.

Sparingly foluble in alcohol,

Like the two kinds of wax, it is foluble in alcohol, though very fparingly; according to my experiments, it required a quantity of boiling alcohol equal to 150 times the weight of the fpermaceti to diffolve it; a proportion which nearly coincides with the eftimate of Fourcroy: the whole is precipitated as the fluid cools.

rapidly by hot ether,

It is rapidly diffolved by warm ether, by cooling it is precipitated fo plentifully, as in appearance to convert the whole into a folid cryftallized mafs.

and oil of turpentine.

Spermaceti is alfo diffolved with great facility by oil of turpentine gently heated, but is depofited from it as it cools.

More ftrongly attacked by pot-afh than m. or b. wax.

It unites very readily with cauftic potafh, and the compound is completely

* Encyc. Math. Chimie, Art. Blanc de Baleine.
† Annales de Chim. tom. vii. 192.
‡ Nicholfon's Journal, Vol. 1. 4to, p. 70.

foluble

ſoluble in warm water: potaſh ſeems to exerciſe upon ſpermaceti a more powerful action, than upon either the myrtle or the bees wax. Ammoniac, at the uſual temperature of the atmoſphere, does not appear to exerciſe any action upon ſpermaceti, but when boiling it unites with it readily, and forms an emulſion, which is not decompoſed by the cooling of the mixture, or by the addition of water; but the ſpermaceti is inſtantly precipitated by the addition of an acid. No important phenomena reſult from the action of the mineral acids upon ſpermaceti.

Strongly by hot ammonia, and then not decompoſ. by cooling or dilution, but by an acid.

Adipocire.

Adipocire. Deſcription.

I procured a quantity of this ſubſtance by digeſting diluted nitric acid upon the muſcular fibre; it was afterwards waſhed in warm water, in order to ſeparate any portion of adhering acid. The matter thus purified, was of a light yellow colour, of about the conſiſtence of tallow, and of a homogeneous texture. Reſpecting the temperature at which it is fuſed, we meet with the ſame uncertainty as in the former caſes. Fourcroy in one of his eſſays * fixes its melting point at the 98th degree; the ſame author in another place ſtates it to be the 110th †, while Mr. Nicholſon ‡ ſuppoſes it to be as high as the 127th; in Dr. Rees's Cyclop. § it is ſtated, that this ſubſtance melts at 7° below ſpermaceti, which according to my eſtimate would be the 105th degree. In my own experience upon this ſubject it became liquid at the 92d degree. Alcohol, at the ordinary temperature of the atmoſphere, diſſolves it only in ſmall quantity, but by the aſſiſtance of a gentle heat it acts upon it with rapidity. Fourcroy || ſtates that this fluid when boiling diſſolves about its own weight of adipocire, $\frac{1}{4}$ or $\frac{1}{5}$ of which is retained after the fluid cools. The ſame chemiſt in another memoir aſſerts, that one ounce of alcohol will diſſolve 12 drams of this ſubſtance **. There may probably be ſome difference in the chemical nature of adipocire, according

Fuſibility.

Solubility in hot alcohol, conſiderable.

* Ann, de Chimie, tom. vii. 192.

† Ann de Chimie, tom. viii. 66.

‡ Nicholſon's Journal, ubi ſupra.

§ Rees's Cyclop. new edit. Art. Adipocire, this is probably inſerted only upon the authority of Fourcroy.

|| Ann. de Chimie, VII. 191.

** Ann. de Chimie, VIII. 67.

to

to the procefs by which it is obtained, or the rapidity of its production; in my experiments the quantity which the alcohol was capable of diffolving, though very confiderable, was certainly lefs than that ftated by Fourcroy. The greater part is depofited as the fluid cools, and the remainder may be precipitated by water. The adipocire, after this operation, is rendered nearly white, while the alcohol affumes a deep yellow tinge. Ether diffolves it fparingly, when unaffifted by heat; when boiling it takes up about $\frac{1}{4}$ of its own weight; this is for the moft part feparated by the cooling of the fluid; the adipocire is depofited nearly white, while the ether acquires a yellowifh green colour. The cauftic alcalis, both fixed and volatile, exert upon this fubftance the fame kind of action which we have defcribed in the former inftances; when heated in contact with it, they form a faponaceous emulfion of a reddifh brown colour, which is mifcible with water without decompofition. The volatile alcali diffolves it fparingly, without the affiftance of heat; a circumftance in which the adipocire differs both from any of the fubftances which we have hitherto examined, and alfo from the cryftalline matter of biliary calculi. Upon the whole the adipocire is more fufible, more inflammable, and more eafily acted upon by the different reagents than any fubftance which has paffed under our review.

and in ether.

Alkalis as before.

Ammonia diffolves it cold.

Cryftalline Matter of Biliary Calculi.

Cryftals of biliary calculi. Defcription.

It now only remained to perform fome comparative experiments with the cryftalline matter of biliary calculi, and I was fortunately in poffeffion of two of thefe bodies, which were prefented to me by Dr. Gerard of this place. The calculi were fimilar in their texture and appearance, and of nearly the fame fize; the one which I examined was of an irregular, polyhedral figure, with its edges and angles blunted; it was fomewhat brittle, and of an ochry brown colour. Its fpecific gravity was about ,9000; it weighed 16 grains. When broken, and viewed through a microfcope, it was found to confift of an internal nucleus, formed of radii converging to a centre, and of an external cruft compofed of four or five thin ftrata. It evidently confifted of two diftinct fubftances; one white, fparkling, and of a cryftalline texture, by which its general ftructure was determined; the other a number of dark coloured particles, irregularly difperfed through the interftices of

of the former. I therefore concluded it to be that kind of calculus which Fourcroy calls Cystic-Adipobileous *; composed of the peculiar crystallized matter, and of particles of inspissated bile, mixed together in different proportions. The crystalline matter is fusible and inflammable, but the precise degree at which it melts has not been ascertained; it is not even softened by the heat of boiling water. Alcohol in the cold has no action upon it, but when boiling it dissolves it with facility; Fourcroy † states, that one part of this substance is taken up by 19 parts of the fluid; this however was not the case with the calculus which I examined; in this instance the alcohol certainly did not dissolve more than $\frac{1}{30}$ of its weight of the crystalline matter. As the fluid cools, the substance is deposited in the form of white, shining spiculæ, intermixed with thin plates. Ether dissolves it slowly in the cold, but more readily when heated; the greatest part is deposited as the fluid cools, and the rest may be precipitated by water. If the ether be suffered to evaporate slowly at the ordinary temperature of the atmosphere, the matter which it held in solution will be deposited on the sides of the glass, in the form of beautiful radiated crystals. Oil of turpentine acts upon this matter with difficulty; it appears, however, when digested with it for some time at the boiling heat, to dissolve it in a small degree. It is acted upon by caustic potash when boiling, and the result of their union appears to be of the same nature with that described in the former instances. A small quantity is dissolved by the pot-ash, and may be precipitated by an acid; while another part is converted into a substance soluble in water, but insoluble in alcohol; it may be precipitated from the water by an acid. Ammoniac, even when boiling, seems to possess little or no attraction for this crystalline matter.

Fusible and inflammable; not acted on by water.

Hot alcohol dissolves it sparingly,

ether more fully.

Ol. turpentine little.

Fixed alkalis as before, and ammonia scarcely at all.

The results of several experiments oblige me to differ from Dr. Powel, respecting the effect of the fixed alcalis upon this peculiar substance; he conceives that it is not acted upon by them ‡; the opinion which I have adopted is however supported by the authority of Fourcroy §. Nitric acid, more particularly

Nitric acid acts upon and alters it.

* Fourcroy, Système des Conn. Chim. x. 59.

† Ann. de Chimie, VII.

‡ Powel on the Bile, 119.

§ Encyc. Meth. Chimie; art. Bile; Fourcroy, Système des Conn. Chimiques, x.

particularly when affifted by heat, acts readily upon the cryftalline matter; during the procefs there is a difengagement of nitrous gas. A fmall quantity remains diffolved in the fluid, and may be precipitated from it by potafh. The greateft part however rifes to the furface as the fluid cools in the form of drops of oil, which gradually grow concrete; the cryftalline texture is deftroyed, and its confiftence refembles that of a vegetable refin. Water does not diffolve this peculiar matter, but it feems to render it fomewhat more brittle and friable. Alcohol affifted by a gentle heat diffolved it; it was precipitable from the folution by water in the form of a grey powder. Ether, at the temperature of the atmofphere, diffolved it rapidly; water precipitated it from the ether in the form of drops of oil. When the fluid was evaporated, it was depofited, without exhibiting any marks of a cryftalline ftructure. Cauftic potafh acted upon it without the affiftance of heat, when boiling it diffolved it with more facility; the fluid acquired a reddifh brown hue; it was not precipitated by water, but the fulphuric acid feparated it in the form of a grey powder. The action of ammoniac was nearly fimilar, though as I conceived, fomewhat more powerful than that of the fixed alcalis. The folution was alfo of a reddifh brown colour; but the precipitate by fulphuric acid was of a bright yellow. It appears therefore that the matter of biliary calculi has its properties materially changed by the operation of the nitric acid. It entirely deftroys its cryftalline tendency, and renders it more foluble in ether and in the alcalis. It has been fuppofed that by this procefs it becomes more affimilated to the adiporefin of the bile *, but it ftill differs from it in not poffeffing any degree of folubility in water †.

Habitudes of the altered fubftances.

These remarks upon the cryftalline matter of biliary calculi, fhew that there are feveral important circumftances in which it differs both from fpermaceti, and from adipocire, to both of which it has been compared ‡. Upon the whole, though the

The fubftances here treated are really different.

* Powel, p. 121.

† Ann. de Chimie, tom. vii. p. 178. Encyc. Meth. Chimie, art. Bile, p. 566.

‡ I may obferve that the dark-coloured particles which were difperfed through the cryftals of the calculus, though they might have been originally compofed of infpiffated bile, did not retain the properties of this fubftance, as they were nearly infoluble both in boiling water and in alcohol.

five

five ſubſtances which have paſſed under our review poſſeſs certain properties in common, and have a degree of ſimilarity in their external appearance, yet they differ materially in their chemical nature. There is indeed reaſon to conjecture that they are all compoſed of the ſame elements, combined together in different proportions and with different degrees of attraction.

Lavoiſier * firſt made us acquainted with the chemical compoſition of oil, and proved that it conſiſts of hydrogene and carbone. This great philoſopher alſo demonſtrated that wax differs from oil, in containing a greater proportion of carbone; there is every reaſon to ſuppoſe, that a quantity of oxygene likewiſe enters into its compoſition. It may be conjectured, that the five ſubſtances which have paſſed under our review, differ from each other in the proportion of oxygene, hydrogene, and carbone; but the preſent ſtate of our knowledge will not enable us to determine how far the individual properties will be affected by the different proportions. It had been conjectured, that an addition of carbone renders a body leſs fuſible, and at the ſame time more ſoluble in alcohol; but we find from the experiments recited above, that adipo-cire, which is the moſt fuſible, is likewiſe the moſt ſoluble in alcohol. Probably a good deal may depend in theſe caſes upon the ſtate of the combination of the ingredients, as well as upon their proportions. General remarks.

II.

On the Nature of Muſical Sounds. In Reply to DR. YOUNG. *By Mr.* JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

THE controverſy between Dr. Young and me has taken a new turn in your laſt number; for my opponent, with the addreſs of an able politician, endeavours to ſecure the victory by the aid of a powerful auxiliary. For this purpoſe, he has attempted to oppreſs my cauſe, and ſilence my reaſons, by the authority of an illuſtrious name; whom he has impreſſed into Introductory remark.

* Lavoiſier, Mem. Acad. Scienc. 1784.

his

his ſervice. The only plea that I can offer in oppoſition to this formidable confederacy, conſiſts in the following plain remark: I contend for the truth and not for ſuperiority, a motive which obliges every diſputant to diſregard all authority, except the conviction impreſſed on the ſenſes by a judicious experiment, and the aſſent given by the underſtanding to ſound arguments. Notwithſtanding the preceding declaration, I acknowledge the weight of perſonal reſpectability, in many inſtances of an experimental nature; becauſe the reputation of one philoſopher ſtands higher than that of another does, as a conductor of experiments; on this ſuppoſition, the deciſions of the former ought to be preferred to thoſe of the latter, when they happen to diſagree in their concluſions; in fact every man's confidence in the juſtice of an experiment which he has not ſeen, amounts to nothing more than hiſtorical evidence, the credit of the narrative reſting entirely upon the character and abilities of the eye witneſs.

Perſonal reſpectability is of weight in hiſtory;

but not in argument or reaſoning.

The foregoing obſervations on experimental knowledge have nothing to do with argument; for every ſpecies of true logic, including the mathematics, conſiſts of a train of inductions; all of which are drawn from maxims, admitted by every party, in a well conducted diſpute. On this account, argumentation is not an appeal to a perſon's faith, but to his rational faculties; and an unprejudiced judge will prefer the logic of a ploughman to that of Ariſtotle himſelf, ſhould the ruſtic happen to reaſon better than this maſter of the art. After what has been ſaid on the weight of authority in philoſophical controverſy, it will not be expected that I ſhall pay the leaſt deference to the dogma of any man in the world, unleſs his opinion is ſupported by demonſtration; for this is the only literary authority to which an enquirer after the truth ought to bow. Conſiſtently with this declaration, an attempt has been made to demonſtrate every propoſition that has been advanced on my part, in the preſent diſpute; a diſpute which commenced in the tranſactions of the Royal Society, which paſſed from that publication to the Mancheſter Memoirs, and is now prolonged, Sir, in your Journal. But my care in this reſpect, and attachment to proof have not been imitated by my opponent; for nothing appears in the courſe of the controverſy, reſembling a refutation of any one of my concluſions, excepting an objection which was offered in Dr. Young's laſt letter, to

The preſent controverſy not determinable by authority.

to the theory of the grave harmonics. The Doctor obferves, that two unifons, the vibrations of which bifect each other, ought to give the octave, according to my principles. To this I take the liberty to reply, that no two perfect unifons produce a cycle; becaufe the arrangement which is given to their firft vibrations, will evidently prevail to the end of the experiment: no points of divifion can therefore be formed for the feparation of the contiguous cycles; in other words, no cycles will be produced, confequently no imaginary found can be perceived in the courfe of the Doctor's experiment, becaufe the exiftence of the grave harmonics depends upon a fucceffion of minute cycles, if my idea of their origin be correct. The effect of two imperfect unifons will perhaps appear upon examination, not lefs unfavourable to the Doctor's objection, than the preceding cafe: confonances of this defcription, it is true, always run into cycles; but then they are too long to give rife to continuous founds, being better fitted to produce beats. For inftance, let two homogeneous cords of equal diameters be ftretched with equal forces; and fuppofe their lengths to be 20 and 20,1 inches, then the longer will make 200 vibrations, while the fhorter compleats 201. But the cord correfponding to the grave harmonic of this confonance, vibrates but once in the fame time; therefore the length of it is to 20,1 inches as 200 to 1; or it is 335 feet. The difficulty of conducting fuch an experiment muft be obvious to every one; for whether ftrings or pipes be ufed, the lower note of this confonance muft compleat at leaft twelve cycles, or 2400 vibrations in a fecond; otherwife a grave harmonic cannot refult from this combination of imperfect unifons.

That two unifons cannot give the octave by bifection, infifted becaufe they cannot form a cycle.

Imperfect unifons produce beats.

Having now replied to the Doctor's objection, apparently in a fatisfactory manner, I may without the imputation of rudenefs, requeft him to confider in what way the feveral properties of thefe fecondary founds are to be reconciled to his fyftem, more efpecially their want of a fixed direction: fhould he fail in the attempt, he needs not be reminded that every theory is imperfect, which explains a part of a phenomenon, but does not embrace the whole of it. The want of direction appeared to me fo plain a proof of the nature of the grave harmonics, the firft time I made the experiment, that I immediately explained the fact on the principles, which are given at the beginning of this volume of your Journal, without once fuppofing the theory

How the want of direction, &c. can be reconciled to Dr. Young's fyftem?

to be new; before Dr. Young's laſt letter informed me of the circumſtance. Every attempt to account for this ſingular effect of conſonances by pulſes tranſmitted to the ear through the air, will in all probability encounter the obſtacle already mentioned: for the notion of force compriſes the idea of direction, as well as that of power; and if the latter property cannot be uſed independent of the former, every theory founded on this principle muſt be contradicted by obſervation. I do not pretend to form a judgment on the work of M. Lagrange, from Dr. Young's extracts; and perſons reſiding at a diſtance from the metropolis, as I do, ſeldom enjoy acceſs to uncommon books, ſuch as the Miſcellanea Taurinenſia. However, I agree to the propoſition of M. Lagrange, that a particle of air agitated by two ſounds, acquires a motion differing from that, which each of them would give to it ſeparately: at the ſame time I muſt withhold my aſſent to the ſequel, if I underſtand it rightly; viz. that the ſounds coaleſce in conſequence of this motion, until my arguments on this ſubject have been refuted. While this remains unattempted, the prolongation of the preſent controverſy promiſes to be of no advantage to ſcience: I will therefore in future diſregard all remarks and objections which do not attack the fundamental principle of my theory.

JOHN GOUGH.

Middleſhaw near Kendal, Feb. 11, 1803.

III

Obſervations on the two lately diſcovered celeſtial Bodies. By WILLIAM HERSCHEL, *LL. D. F. R. S.*

(Concluded from Page 128.*)*

WITH regard to the fifth, concerning ſatellites, it may not be eaſy to prove a negative; though even that, as far as it can be done, has been ſhewn. But the retention of a ſatellite in its orbit, it is well known, requires a proper maſs of matter in the central body, which it is evident theſe ſtars do not contain.

The ſixth article ſeems to exclude theſe ſtars from the condition of planets. The ſmall comas which they ſhew, give them

them ſo far the reſemblance of comets, that in this reſpect we ſhould be rather inclined to rank them in that order, did other circumſtances permit us to aſſent to this idea.

In the ſeventh article, they are again unlike planets; for it appears, that their orbits are too near each other to agree with the general harmony that takes place among the reſt; perhaps one of them might be brought in, to fill up a ſeeming vacancy between Mars and Jupiter. There is a certain regularity in the arrangement of planetary orbits, which has been pointed out by a very intelligent aſtronomer, ſo long ago as the year 1772; but this, by the admiſſion of the two new ſtars into the order of planets, would be completely overturned; whereas, if they are of a different ſpecies, it may ſtill remain eſtabliſhed.

The new ſtars are not planets.

As we have now ſufficiently ſhewn that our new ſtars cannot be called planets, we proceed to compare them alſo with the other propoſed ſpecies of celeſtial bodies, namely, comets. The criteria by which we have hitherto diſtinguiſhed theſe from planets, may be enumerated as follows.

Criterions of comets.

1. They are ſmall.

1. They are celeſtial bodies, generally of a very ſmall ſize, though how far this may be limited, is yet unknown.

2. Orbits excentric.

2. They move in very excentric ellipſes, or apparently parabolic arches, round the ſun.

3. Planes various.

3. The planes of their motion admit of the greateſt variety in their ſituation.

4. Direction various.

4. The direction of their motion alſo is totally undetermined.

5. Great atmoſpheres.

5. They have atmoſpheres of very great extent, which ſhew themſelves in various forms of tails, coma, hazineſs, &c.

Compariſon of the new ſtars in theſe reſpects.

On caſting our eye over theſe diſtinguiſhing marks, it appears, that in the firſt point, relating to ſize, our new ſtars agree ſufficiently well; for the magnitude of comets is not only ſmall, but very unlimited. Mr. Pigott's comet, for inſtance, of the year 1781, ſeemed to have ſome kind of nucleus; though its magnitude was ſo ill defined, that I probably overrated it much, when, November 22, I gueſſed it might amount to 3 or 4″ in diameter. But, even this, conſidering its nearneſs to the earth, proves it to have been very ſmall.

That of the year 1783, alſo diſcovered by Mr. Pigott, I ſaw to more advantage, in the meridian, with a 20-feet reflector. It had a ſmall nucleus, which, November 29, was coarſely eſtimated to be of perhaps 3″ diameter. In all my other pretty

numerous

numerous obſervations of comets, it is expreſsly remarked, that they had none that could be ſeen. Beſides, what I have called a nucleus, would ſtill be far from what I now ſhould have meaſured as a diſk; to conſtitute which, a more determined outline is required.

In the ſecond article, their motions differ much from that of comets; for, ſo far as we have at preſent an account of the orbits of theſe new ſtars, they move in ellipſes which are not very excentric.

Nor are the ſituations of the planes of their orbits ſo much unlike thoſe of the planets, that we ſhould think it neceſſary to bring them under the third article of comets, which leaves them quite unlimited.

In the fourth article, relating to the direction of their motion, theſe ſtars agree with planets, rather than with comets.

The fifth article, which refers to the atmoſphere of comets, ſeems to point out theſe ſtars as belonging to that claſs; it will, however, on a more particular examination, appear that the difference is far too conſiderable to allow us to call them comets.

Account of the ſize of tails of comets.

The following account of the ſize of the comas of the ſmalleſt comets I have obſerved, will ſhew that they are beyond compariſon larger than thoſe of our new ſtars.

Nov. 22, 1781. Mr. Pigott's comet had a coma of 5 or 6′ in diameter.

Nov. 29, 1783. Another of Mr. Pigott's comets had a coma of 8′ in diameter.

Dec. 22, 1788. My ſiſter's comet had a coma of 5 or 6′ in diameter.

Jan. 9, 1790. Another of her comets was ſurrounded by hazineſs of 5 or 6′ in diameter.

Jan. 18, 1790. Mr. Mechain's comet had a coma of 5 or 6′ in diameter.

Nov. 7, 1795. My ſiſter's comet had a coma of 5 or 6′ in diameter.

Sept. 8, 1799. Mr. Stephen Lee's comet had a coma of not leſs than 10′ in diameter, and alſo a ſmall tail of 15′ in length.

From theſe obſervations, which give us the dimenſions of the comas of the ſmalleſt comets that have been obſerved with good inſtruments, we conclude, that the comas of theſe new ſtars, which at moſt amount only to a few times the diameter of

Dr. Wollaston's Method of examining the refractive & dispersive powers of Bodies.

Fig 1.

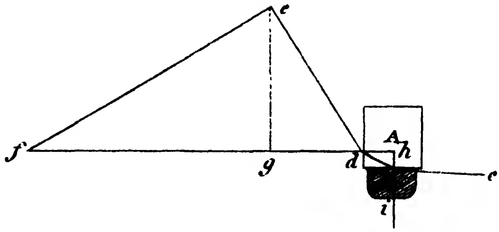


Fig 2.

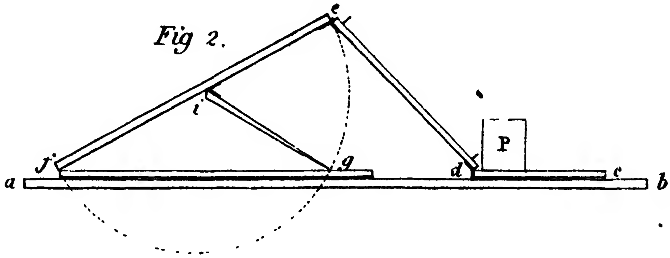
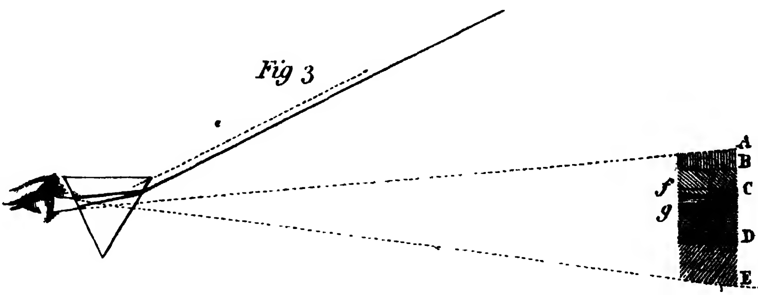
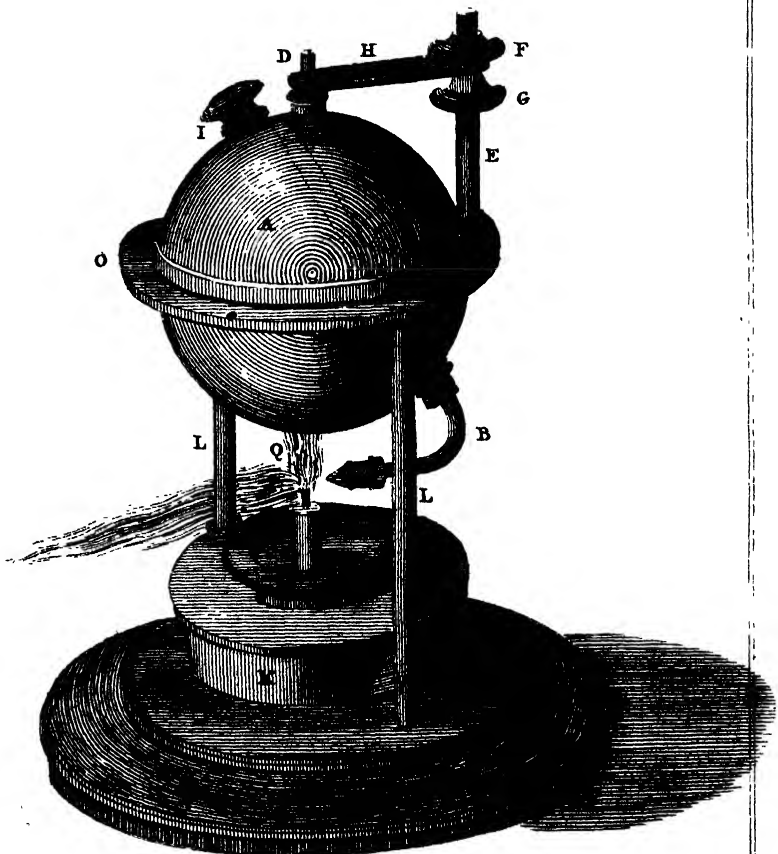


Fig 3



Medlow, Sc Russell Ct

Blow Pipe, by Alcohol. By Mr. Hooke.



Dr. Young's Harmonic Sliders.

Fig. 1.

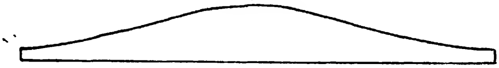


Fig. 2.

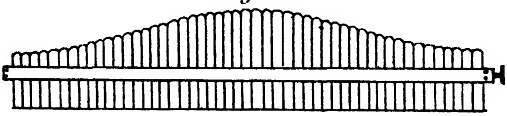


Fig. 3.

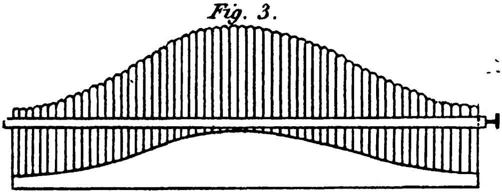


Fig. 4.

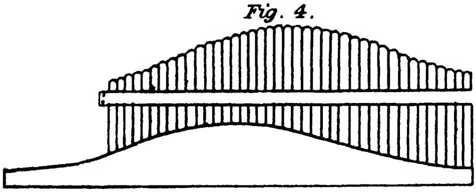


Fig. 5.

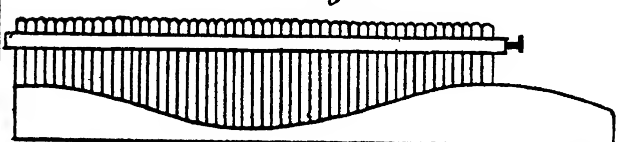
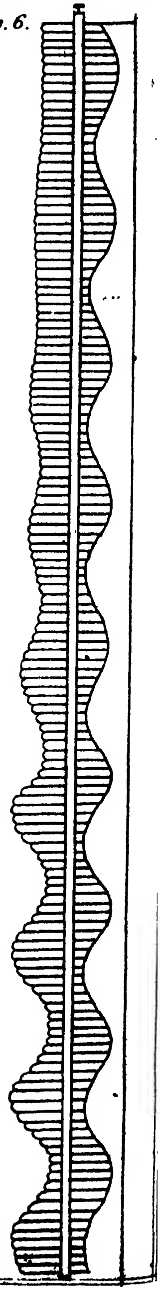


Fig. 6.



Steam cocks, Joints & Tubes N°. 1

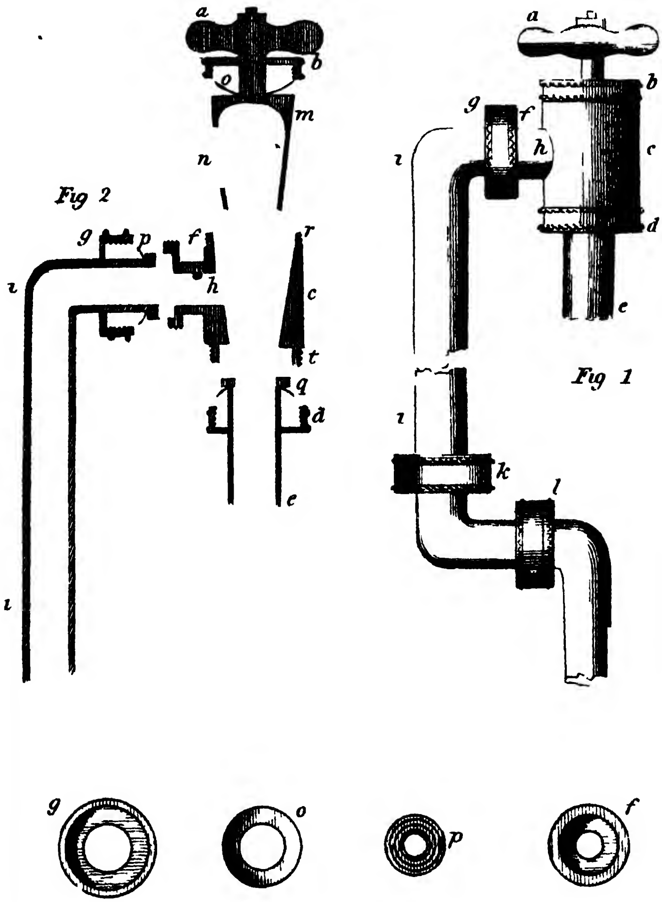


Fig 2

Fig 1

Marlow Sc. Russell Ct

of the bodies to which they belong, bear no reſemblance to the comas of comets, which, even when ſmalleſt, exceed theirs above a hundred times. Not to mention the extenſive atmoſpheres, and aſtoniſhing length of the tails; of ſome comets that have been obſerved, to which theſe new ſtars have nothing in the leaſt ſimilar.

Since, therefore, neither the appellation of planets, nor that of comets, can with any propriety of language be given to theſe two ſtars, we ought to diſtinguiſh them by a new name, denoting a ſpecies of celeſtial bodies hitherto unknown to us, but which the intereſting diſcoveries of Mr. Piazzi and Dr. Olbers have brought to light.

Theſe new ſtars cannot with propriety of language be called planets or comets.

With this intention, therefore, I have endeavoured to find out a leading feature in the character of theſe new ſtars; and, as planets are diſtinguiſhed from the fixed ſtars by their viſible change of ſituation in the zodiac, and comets by their remarkable comas, ſo the quality in which theſe objects differ conſiderably from the two former ſpecies, is that they reſemble ſmall ſtars ſo much as hardly to be diſtinguiſhed from them, even by very good teleſcopes. It is owing to this very circumſtance, that they have been ſo long concealed from our view. From this, their aſteroidical appearance, if I may uſe that expreſſion, therefore, I ſhall take my name, and call them *Aſteroids*: reſerving to myſelf, however, the liberty of changing that name, if another, more expreſſive of their nature, ſhould occur. Theſe bodies will hold a middle rank, between the two ſpecies that were known before; ſo that planets, aſteroids, and comets, will in future comprehend all the primary celeſtial bodies that either remain with, or only occaſionally viſit, our ſolar ſyſtem.

The author propoſes to call them *Aſteroids*.

I ſhall now give a definition of our new aſtronomical term, which ought to be conſiderably extenſive, that it may not only take in the aſteroid Ceres, as well as the aſteroid Pallas, but that any other aſteroid which may hereafter be diſcovered, let its motion or ſituation be whatever it may, ſhall alſo be fully delineated by it. This will ſtand as follows.

Aſteroids are celeſtial bodies, which move in orbits either of little or of conſiderable excentricity round the ſun, the plane of which may be inclined to the ecliptic in any angle whatſoever. Their motion may be direct, or retrograde; and they may or may not have conſiderable atmoſpheres, very ſmall comas, diſks, or nuclei.

Definition of aſteroids.

Others will foon be found out;

As I have given a definition which is fufficiently extenfive to take in future difcoveries, it may be proper to ftate the reafons we have for expecting that additional afteroids may probably be foon found out. From the appearance of Ceres and Pallas it is evident, that the difcovery of afteroids requires a particular method of examining the heavens, which hitherto aftronomers have not been in the habit of ufing. I have already made five reviews of the zodiac, without detecting any of thefe concealed objects. Had they been lefs refembling the fmall ftars of the heavens, I muft have difcovered them. But the method which will now be put in practice, will completely obviate all difficulty arifing from the afteroidical appearance of thefe objects; as their motion, and not their appearance, will in future be the mark to which the attention of obfervers will be directed.

by obferving the relative motion as well as the appearance of ftars.

Society for that purpofe.

A laudable zeal has induced a fet of gentlemen on the Continent, to form an affociation for the examination of the zodiac. I hope they will extend their attention, by degrees, to every part of the heavens; and that the honourable diftinction which is juftly due to the fuccefsful inveftigators of nature, will induce many to join in the meritorious purfuit. As the new method of obferving the zodiac has already produced fuch interefting difcoveries, we have reafon to believe that a number of afteroids may remain concealed; for, how improbable it would be, that if there were but two, they fhould have been fo near together as almoft to force themfelves to our notice. But a more extended confideration adds to the probability that many of them may foon be difcovered. It is well known that the comas and tails of comets gradually increafe in their approach to the fun, and contract again when they retire into the diftant regions of fpace. Hence we have reafon to expect, that when comets have been a confiderable time in retirement, their comas may fubfide, if not intirely, at leaft fufficiently to make them affume the refemblance of ftars; that is, to become afteroids, in which ftate we have a good chance to detect them. It is true that comets foon grow fo faint, in retiring from their perihelia, that we lofe fight of them; but, if their comas, which are generally of great extent, fhould be compreffed into a fpace fo fmall as the diameters of our two afteroids, we can hardly entertain a doubt but that they would again become vifible with good telefcopes. Now, fhould we fee a comet in its aphelion, under the conditions here pointed out, and that there are many which may be in fuch fituations, we have the greateft

Comets may become afteroids;

and be obferved in their aphelions.

greateſt inducements to believe, it would be a favourable circumſtance to lead us to a more perfect knowledge of the nature of comets and their orbits; for inſtance, the comet of the year 1770, which Mr. Lexell has ſhewn to have moved in an elliptical orbit, ſuch as would make the time of its periodical return only about $5\frac{1}{2}$ years: if this ſhould ſtill remain in our ſyſtem, which is however doubtful, we ought to look for it under the form of an aſteroid.

If theſe conſiderations ſhould be admitted, it might be objected, that aſteroids were only comets in diſguiſe; but, if we were to allow that comets, aſteroids, and even planets, might poſſibly be the ſame ſort of celeſtial bodies under different circumſtances, the neceſſary diſtinction ariſing from ſuch difference, would fully authoriſe us to call them by different names.

It is to be hoped that time will ſoon throw a greater light upon this ſubject; for which reaſon, it would be premature to add any other remarks, though many extenſive views relating to the ſolar ſyſtem might certainly be hinted at.

Additional Obſervations relating to the Appearances of the Aſteroids Ceres and Pallas.

Additional obſervations.

May 4, 12h 40′. 10-feet reflector; power $516\frac{1}{2}$. I compared Ceres with two fixed ſtars, which, in the finder, appeared to be of very nearly the ſame magnitude with the aſteroid, and found that its coma exceeds their aberration but in a very ſmall degree.

The comas do not much exceed thoſe of the fixed ſtars.

12h 50′. 20-feet reflector; power 477. I viewed Ceres, in order to compare its appearance with regard to hazineſs, aberration, atmoſphere, or coma, whatever we may call it, to the ſame phenomena of the fixed ſtars; and found that the coma of the aſteroid did not much exceed that of the ſtars.

I alſo found, that even the fixed ſtars differ conſiderably in this reſpect among themſelves. The ſmaller they are, the larger in proportion will the attendant hazineſs ſhew itſelf. A ſtar that is ſcarcely perceptible, becomes a ſmall nebuloſity.

10-feet reflector. 13h 10′. I compared the appearance of Pallas with two equal fixed ſtars; and found that the coma of this aſteroid but very little exceeds the aberration of the ſtars.

14h 5′, 20-feet reflector. I viewed Pallas; and, with a magnifying power of 477, its diſk was viſible. The coma of this aſteroid is a little ſtronger than that which fixed ſtars of the ſame ſize generally have.

 On

IV.

On the oblique Refraction of Iceland Crystal. By WILLIAM HYDE WOLLASTON, *M. D. F. R. S. From the Phil. Transf. for* 1802.

IN the preceding communication *, I have inferted two different meafures of refractive powers, diftinctly obfervable in the Iceland cryftal, as well as an eftimate of its difperfive power; but have referved for a feparate treatife, fome renarks which the fame mode of inveftigation has enabled me to make on its oblique refraction.

Iceland cryftal well examined by Huygens.

The optical properties of this body have been fo amply defcribed by Huygens in his *Traité de la Lumière,* that it could anfwer little purpofe to attempt to make any addition to thofe which he has enumerated. But, as the law to which he has reduced the oblique refractions occafioned by it, could not be verified by former methods of meafurement, without confiderable difficulty, it may be worth while to offer a new and eafy proof of the juftnefs of his conclufions. For, fince the theory by which he was guided in his inquiries. affords (as has lately been fhown by Dr. Young †) a fimple explanation of feveral phenomena not yet accounted for by any other hypothefis, it muft be admitted that it is entitled to a higher degree of confideration than it has in general received.

His theory, that light is propagated by vibrations of a medium; undulating fpherically in moft cafes; but in an oblate fpheroid, in Iceland cryftal:

According to that hypothefis, light proceeding from any luminous centre, is propagated by vibrations of a medium highly elaftic, that pervades all fpace. In ordinary cafes, the incipient undulations are of a fpherical form; but, in the Iceland cryftal, light appeared to Huygens to proceed as if the undulations were portions of an oblate fpheroid, of which the axis is parallel to the fhort diagonal of an equilateral piece of the cryftal and its centre the point of incidence of the ray.

Whence the law of this refraction: viz that the fine of incidence has a conftant rario to the ordinate of refraction in the fpheroid.

From this fpheroidical form of the undulations, he deduces the obliquity of refraction; and lays down a law, obfervable in all refractions, at any furface of the fpar, whether natural or artificial, which bears the clofeft analogy to that which obtains univerfally at other refracting furfaces; for as, in other

* See our Journal IV. 89.

† Bakerian Lecture. Phil. Trans. for 1801.

cafes,

cafes, the ratio is given between the fine of incidence and fine of refraction, (or ordinate of the *fpherical* undulation propagated,) fo, in the Iceland cryftal, the ratio between the fine of incidence and ordinate of refraction (in any one fection of the *fperoidical* undulation) is a given ratio.

Illuftration by diagram.

If ABD (Fig. 1, Plate VIII.) be any furface of the fpar, let FHOK be a fection of the fpheroid through its centre C, and RC any ray of light falling on that furface; draw FO a diameter of the fpheroid, in the plane of incidence RVO, and CT, its femiconjugate diameter, in the plane of refraction FTO. Then, if CI be the refracted ray, VR, the fine of incidence, fhall be to EI, the ordinate of refraction parallel to FC, in the conftant ratio of a given line N to the femidiameter FC.

But the refraction will differ, in different planes of incidence, as the diameter of the ellipfe, &c.

In any other plane of incidence, the ratio of fine to ordinate is alfo conftant; but it is a different ratio, according to the magnitude of that diameter in which the plane of incidence interfects the ellipfe FHOK.

as may be feen by the angle of total reflection.

When the incidence of the ray paffing from any medium of greater denfity upon the furface of this fpar, is fuch that the emergent ray becomes parallel to the furface, the ordinate of refraction is then a femidiameter of the fpheroid; and, accordingly, the refractive power of this fpar, when examined by means of a prifm in different directions, fhould be found to vary as that femidiameter which coincides with the plane of incidence and refracting furface.

Hypothefis of Huygens confirmed.

The obfervations that I have made on this fubftance, accord throughout with this hypothefis of Huygens; the meafures that I have taken correfpond more nearly than could well happen to a falfe theory, and are the more to be depended on, as all my experiments, excepting the laft, were made prior to my acquaintance with the theory, and their agreement was deduced by fubfequent computation.

Experiments on the oblique refraction in different planes.

Exp. 1. The oblique refraction of this fpar is rendered vifible, by cementing a furface of it to a prifm of flint-glafs, with a little balfam of Tolu. When the line of fight befects an acute angle of a natural furface of the fpar, the refractive power is feen to be lefs than in any other direction, and may be expreffed by the fine 1,488, or its reciprocal 0,67204.

Exp. 2. When the plane of incidence is parallel to one of the fides, the power is 1,518, of which the reciprocal is 0,6587.

Exp.

Exp. 3. In a direction at right angles with either fide, it is found ftill higher, being 1,537, or its reciprocal 0,6506.

Exp. 4. And in the plane bifecting an obtufe angle, the refractive power of the natural furface appears greater, and is expreffed by the fine 1,571, or its reciprocal 0,6365.

and furfaces.

Exp. 5. When either of the two greateft folid angles of the fpar contained under three obtufe angles, is cut off by a polifhed furface making equal angles with each of its fides, the fame refractive power 1,488 is found in all directions. By the theory alfo, the fection of the fpheroid is in this cafe a circle, and every femidiameter (FC) the fame, fince the plane is at right angles to the minor axis.

Exp. 6. If a plane furface be formed bifecting an obtufe angle of the fpar, and applied to a prifm, the fame minimum of refraction 1,488, is found in a direction that coincides with the preceding plane, and therefore with the major axis of the generating ellipfe; but, as the direction is varied, it increafes fo rapidly as foon to exceed the power of glafs, and to be no longer afcertainable by the angle of incipient reflection.

Regular refraction.

Exp. 7. The regular refraction of this fpar is alfo too great for examination by means of any prifm, for want of a medium of union of fufficient denfity; but, by trial in the ufual method, it meafured, on an average of feveral experiments, 1,657, or its reciprocal 0,6035.

Spheroid which regulates the refractions.

By affuming, as Huygens has done, the equality of this power with the maximum of the oblique refraction, we have fufficient data for conftruction of the fpheroid by which the refractions are regulated; for we have 0,67204 (*Exp.* 1.) as major axis of the generating ellipfe, and 0,6035 (*Exp.* 7) will be the minor axis, parallel in pofition to the fhort axis of the fpar.

The angle of inclination of this axis to the furfaces of the fpar, if fuppofed to be equilateral, may be computed by fpherical trigonometry, from any other angle that has been afcertained by meafurement.

The meafures that I have taken are not exactly thofe of Huygens; but I neverthelefs hold them in equal eftimation, from the conformity which I find they bear to each other, by affiftance of his theory.

Deduction of its dimenfions and pofition.

Exp. 8. I meafured with care an angle at which two furfaces of the fpar are inclined to each other, and found it to be 105° 5′.

Hence

Hence, the greater angle of the furfaces themfelves may be computed to be 101° 55′; and the angle which the fhort axis makes with each plane furface is 45° 23′ 25″.

If GSMP (Fig. 2.) be a plane bifecting an obtufe angle of the fpar, the fection of the fpheroid in that plane paffes through the axis CS, and therefore is the generating ellipfe. By calculating from the known dimenfions of its major axis CP 0,67204, its minor axis CS 0,6035, and the angle GCS=45° 23′ 25″, CG will be found * to be 0,6365, of which the reciprocal is 1,5736, differing but little from 1,571, as it appeared by meafurement. (*Exp.* 4.)

Again, if ABDE (Fig. 4.) be one of the natural furfaces, and PG*p* the ellipfe formed by that fection of the fpheroid, PC being as before 0,67204, and CG 0,6365, the reciprocal of 1,571 found by meafurement, (*Exp.* 4) then the femidiameter CT, parallel to the fide AE, which makes an angle TCP 39° 2½′, will be found to be 0,6573, inftead of 0,6587, and its reciprocal 1,5215, inftead of 1,518. (*Exp.* 2.)

The femidiameter alfo, in the direction of CL, perpendicular to the fide, at an angle LCP 50° 57½′, is found by calculation 0,650, and its reciprocal 1,539, inftead of 0,6506 and 1,537. (*Exp.* 3.)

Courfe of a perpendicular ray after this refraction. From the foregoing data, the courfe of a ray perpendicular to the furface of the fpar may likewife be computed; for, fince the fine of incidence is then nothing, the ordinate of refraction muft be alfo nothing, and the ray will be refracted along the femiconjugate diameter CM. (Fig. 2.)

By calculation †, the angle which this conjugate makes with the perpendicular is 6° 7½′. But, by the following meafurement, it appears to be 6° 16′.

Experiment to fhew the fame courfe. *Exp.* 9. A piece of fpar that meafured 1,145 inch in thicknefs, was laid upon a line, and fhowed two images that were removed from each other $\frac{126}{1000}$ of an inch. Then, as 1,145 : 0,126 :: radius : tang. of 6° 16′.

Comparative ftatement of refults from obfervation and from theory. The different refults deduced from theory and from obfervation, will be feen at one view in the following ftatement:

* (Fig. 3.) CS : CP :: tang. PCG : tang. PC*p* fec. PC*p* : fec. PCG :: CP : CG.

† (Fig. 5.) CS : CP :: tang. PCG : tang. *p*CO or co-tang. PCQ; then CP : CS :: tang. PCQ : tang. PCM; and LCP—PCM=MCL.

In

In *Exp.* 2d, obferved 1,518; calculated 1,5215.
3d, ——— 1,537 ——— 1,539.
4th, ——— 1,571 ——— 1,5736.
6th, angle obferved 6° 16′ - - 6° 7½′.

Remarks. The angle obferved differs from that obtained by computation, in a greater degree than any of the former meafures; but, when the difficulty of meafuring this angle with accuracy is confidered, and alfo the greater effect of any incorrectnefs in the data from which a femiconjugate is computed, I think the refult of this, as well of the preceding comparifons, muft be admitted to be highly favourable to the Huygenian theory; and, although the exiftence of two refractions at the fame time, in the fame fubftance, be not well accounted for, and ftill lefs their interchange with each other, when a ray of light is made to pafs through a fecond piece of fpar fituated tranfverfely to the firft, yet the oblique refraction, when confidered alone, feems nearly as well explained as any other optical phenomenon.

V.

The Theory of Compound Sounds, By Mr. JOHN GOUGH *.

Dr. Smith's, theory of compound founds; that the pulfes do not obftruct or become confounded with each other.

DR. SMITH, author of the work on Harmonics, takes for granted in his theory of compound founds, that the pulfes which proceed at the fame time from a number of founding bodies, do not clafh, or obftruct one another, in their paffage through the air. According to this hypothefis, each fet, of any number of cotemporary fets of pulfes, ftrikes the ear without being confounded with the reft; in confequence of which, any number of founds may be diftinctly perceived at the fame time. On this fuppofition, a compound found is a fenfation rendered variable by the irregular manner in which the pulfes of the conftituent founds fucceed to one another. For, if the intervals of time between two fucceffive pulfes of one

* Manchefter Memoirs, v. 653. This treatife which was not inferted at an earlier period in our Journal, on account of the preffure of other matter, has become of ftill more intereft as containing the theory to which the author's late difcuffion with Dr. Young is directed.—W. N.

one of the conſtituent ſounds be not equal to the ſame intervals belonging to the ſound or ſounds which accompany it, the ſecondary intervals, or ſmall parts of time ſeparating the pulſes which fall in ſucceſſion on the ear, will vary in magnitude; in the ſame manner that the diſtances between the figures upon the face of a barometer and its nonius vary, none on the ſlider coinciding with thoſe on the fixed plate excepting the higheſt and loweſt. I have choſen this familiar inſtrument to illuſtrate Dr. Smith's method of explaining the phyſical cauſe of compound ſounds, becauſe it affords a viſible example of a cycle of pulſes, according to his notion of the ſubject.

The intervals of the pulſes are not equal, but increaſe and diminiſh periodically, as in a ſcale and nonius.

The ſketch which I have exhibited of Dr. Smith's hypotheſis ſhews, that he allowed that a number of ſimple ſounds might exiſt in concert, and ſtrike the ear in a diſtinct manner, without ſuffering any interruption in their motions from the interference of their pulſes. But a late writer on ſound rejects this axiom in Harmonics as a mathematical inconſiſtency; and ſubſtitutes the following theory of compound ſounds in the room of it. If two muſical ſtrings, differing in their times of vibration, happen to vibrate in concert, they do not occaſion two diſtinct ſounds in the opinion of this gentleman, becauſe the ſtrings agitate the air in conjunction; conſequently the pulſes, which one of them would actually form in an undiſturbed atmoſphere, muſt unavoidably claſh with thoſe which the other ſtring would produce in ſimilar circumſtances. Hence the waves of air belonging to both ſtrings are interrupted in their natural progreſs, and are compelled by their mutual interference to coaleſce, thereby producing a new ſucceſſion of pulſes, conſtituting a ſingle ſound in the place of two. This ſound is of a peculiar kind; for, the pulſes of which it conſiſts, are ſeparated by unequal intervals of time, and diſpoſed in cycles.

Another theory (Dr. Young's), that the pulſes coaleſce; and form a peculiar ſound, having its pulſes unequal and diſpoſed in cycles.

The merit of the preceding theory, when compared with Dr. Smith's hypotheſis, muſt be aſcertained by contraſting it with a variety of facts, which are furniſhed by the phænomena of compound ſounds, and make a part of every man's experience. For, if it be found upon examination to be repugnant to theſe facts, it will prove inconſiſtent with nature, and cannot fail of diſappointing the inventor's expectations.

Examination of the latter theory.

Were it poſſible for a number of ſounds to coaleſce, and form but one, the compound would acquire ſenſible properties peculiar to

If ſounds could coaleſce they would form a new compound;

to itfelf, and at the fame time lofe the diftinguifhing characters of its elements, fome of which are incompatible with the qualities of an individual. On this fuppofition, the prefence of the conftituent founds could not be detected by the ear in this newly created being: on the contrary, an experimental procefs would be required to analyfe every compound found the firft time it attracted a man's attention, for the fame reafon that a chemift finds it neceffary to analyfe a fubftance with which he is unacquainted. The abftract term *coalefcence* is ufed, in a phyfical fenfe, to fignify any intimate union of bodies or the powers of bodies; and the introduction of the term into language proves the exiftence of the principle in nature, or more properly in the human mind. For, when a number of agents act in conjunction upon one of the fenfes, we have two ways of conceiving their mode of operation. If the fenfible effects of each agent be diftinctly perceived, we attribute a feparate action to every member of the affemblage, and call the aggregate a mixture: this is the conclufion of a perfon who taftes an infufion of pepper in vinegar. On the other hand, when we know that certain agents are prefent without being able to recognize their diftinguifhing powers, in the room of which we find qualities of a different defcription, we pronounce the aggregate to be in a ftate of coalefcence. This is the fituation of the chemift, who taftes common falt, but cannot perceive the prefence of foda and the muriatic acid. It is my bufinefs then to prove compound founds to be *mixtures*, not aggregates by coalefcence. This I fhall endeavour to do, by fhewing that they have properties which belong not to individuals, fuch as a number of tones, a variety of directions, and feveral fets of pulfes.

in which the ear could not diftinguifh the component parts.

Chemical comparifon.

Compound founds are mixtures; not combinations.

Firft, the tones of a flute and violin are as diftinct to fenfe as any two things can be when they are founded feparately; and I appeal to common experience to determine, if they are not equally diftinct when heard in concert. Taking it for granted that the anfwer will be in the affirmative, I pronounce the aggregate to be a mixture of founds in one cafe. Secondly, if a violin found in front of the hearer, and a flute be heard at the fame time in an oblique fituation, the perfon thus circumftanced is able to determine the relative pofitions of the two inftruments, which fhews the aggregate to have two cotemporary directions. It is therefore a mixture of founds, not a fingle

Inftances. The flute and violin perfectly diftinguifhable in concert,

the courfes of the two founds are diftinguifhed when they differ.

a ſingle ſound. Thirdly, I have found by making the experiment, that any number of muſical ſtrings may be made to vibrate by a compound ſound acting upon them, provided this compound be occaſioned by an equal number of ſtrings with the former, having one in the latter ſet in uniſon with each one in the preceding ſet. This is an experimental proof that there are as many ſets of pulſes in an aggregate of ſounds as that aggregate contains elements, becauſe no ſtring whatever is in uniſon with a concord or diſcord. Laſtly, if it were poſſible for ſounds to coaleſce, men would never hear any thing more than one noiſe at one time: The general hum would have varied perpetually from the extinction of exiſting ſounds, and the intruſion of freſh ones; but the human mind would have had no conception of two cotemporary ſounds; becauſe the ear being in that caſe incapable of conveying the complex ſenſation, the idea of ſuch an exiſtence would have tranſgreſſed the ſphere of human knowledge. The preceding arguments are drawn, for the moſt part, from common experience; and they ſhew, that the free paſſage of cotemporary ſounds through the air may be ſafely admitted as an axiom in harmonies. I ſhall therefore proceed to prove the ſame propoſition to be conſiſtent with the doctrine of forces.

A number of ſtrings will be ſet into vibration by the compound ſound of other ſtrings, giving uniſons to each reſpectively.

If ſounds coaleſce we ſhould conſtantly hear one varying noiſe.

The propagation of ſound through the atmoſphere, and the nature of aëreal pulſes are commonly explained in elementary books of natural philoſophy; I ſhall, for this reaſon, enumerate only a few particulars, the recollection of which will be found uſeful.

Mechanical arguments.

Propoſition I. Two contiguous particles of air which are agitated by a vibrating body, either directly or by the intervention of an elaſtic medium, receive two motions from each impulſe; *firſt*, an abſolute motion carries them to a greater diſtance from the ſounding body, and afterwards brings them towards it again, both the progreſs and regreſs being performed in the time of a ſingle vibration: *ſecond*, a relative motion reſulting from the former, compels the two particles to approach and recede alternately, which double motion is alſo accompliſhed in the time of a ſingle vibration.

Prop. 1. Vibration of the particles of air, in ſound.

Propoſition II. Both the abſolute and relative motions are greateſt amongſt thoſe particles which are neareſt the ſounding body, and they diminiſh as the diſtance from that body increaſes; but, in all caſes, the change of its place too ſmall to be perceived

Prop. 2. The particles are not ſenſibly removed.

perceived by the ear, on which account every particle preſerves a fixed poſition in reſpect of this organ and its connections.

Phyſical right line. For each corpuſcle is confined within the circumference of a phyſical right line, the diameter of which is determined by its own abſolute motion.

Prop. 3. A particle in the interſection of two rows of particles conveying different ſounds, will be urged in thoſe lines by forces varying in their ratio.

Propoſition III. If two ſounding bodies, affording different notes, act in conjunction upon the ſame particle, through the media of two right lines of ſimilar particles connecting them with it, this particle will be urged at the ſame inſtant in the direction of theſe lines, by two forces having an inconſtant ratio.

For, let the particle A be urged, by the acuter ſound, in the line SA, and by the graver, in the line TA; (*vide* Fig. 2, Plate IX.); then the contiguous particle V, placed in SA, will approach to, and recede from A more frequently than W, ſimilarly placed in TA, by Prop. I.: conſequently the force of V upon A will vary in a quicker manner than the force of W upon A; but this variation of ratio is limited in time; becauſe it evidently begins and ends with the cycle of the vibrations of the ſounding bodies.

Prop. 4. The right lined motion of the particle neceſſary to form a ſound by coaleſcence is impoſſible.

Propoſition IV. The coaleſcence of two ſounds is impoſſible on mechanical principles.

For, ſuppoſe the thing poſſible; then the coaleſcence of two ſounds requires, that a particle of air ſhould poſſeſs a motion, compounded of the motions which the two ſounding bodies would impart to it ſeparately; and that this compound motion ſhould act in a given right line, for an aſſignable part of time, otherwiſe it could not excite a ſimilar motion in the elaſtic particles occupying that given right line. Let A be ſuch a particle, and let the conſtruction uſed in the laſt propoſition, be retained; conſequently (Principia, Prop. 23, Lib. 2.) VA and AW are in the ratio of the forces that act at any moment in the right lines TA and SA. Make AK as AW, and draw KL parallel to AW, and make it as AV; alſo join AL; then will the particle A be urged in the direction LA at that inſtant. But the ratio of AK to KL varies perpetually, by Prop. III.; therefore the ſpecies of the triangle AKL is equally inconſtant; conſequently the compound force does not act in a given direction for an aſſignable part of time. Now the production and propagation of motion in a given right line requires force

to

to be combined with time, which combination is wanting in the prefent inftance; wherefore the coalefcence of founds is impoffible.

Propofition V. It may be demonftrated from mechanical principles, that a number of diftinct cotemporary founds cannot do otherwife than produce diftinct fenfations. Prop. 5. Diftinct cotemporary founds produce diftinct fenfations.

In order to make the neceffary diagram as fimple as poffible, let the directions of two cotemporary fets of pulfes be reprefented by the right lines SM and TN, lying in the fame horizontal plane, and interfecting in the point A; alfo, let BCD be the horizontal fection of the hearer's head, made by the fame plane; and fuppofe the centre of the axis of hearing to be at O; draw OM, ON perpendicular to SM, TN. Now I have fhewn in the preceding paper *, that if a fet of pulfes move in either of the right lines SM, TN, it will excite a fenfation in that part of the head which is cut off by a vertical plane, paffing through one of the perpendiculars OM, ON. It alfo appears from the laft propofition, that the impulfes of the vibrating bodies, acting in the lines SA, TA, do not compel the particle A to move in any given intermediate direction, as LA. But, according to the fecond propofition, the pofition of the particle A, is fixed in refpect of the planes MO, NO; that is, though the corpufcle actually changes place, in refpect of the geometrical point A, it is always found in the interfection of the phyfical right lines SM, TN. Now the two vibrating bodies continue to act in the directions of thefe right lines, confequently the particle A is conftantly urged in thefe lines by two forces, which, though variable in magnitude, are combined with time; which circumftance enables the corpufcle to tranfmit the impulfes of one body to M, and thofe of the other to N. What has been demonftrated of the particle A, may be affirmed of any other particle, which is the interfection of two right lines parallel to SM, TN; in other words, it may be affirmed of two fets of pulfes; and the fame demonftration may be extended to three fets, &c.

Corollary 1. The fubftance of this and the preceding propofition will apply to all elaftic mediums; hence it happens, that a plate of glafs, &c. in a ftate of vibration, will conduct a foreign found, whilft it produces one of its own; for the fame Thefe propofitions apply to founds conducted through folids, &c.

* Manchefter Mem. v. 342, or Philof. Journal, II. 460.

ſame reaſon, if light be conſidered as a vibrating medium, one particle of the luminous fluid may aſſiſt in tranſmitting two ſenſations.

The directions will not be perceived unleſs they differ by a certain angle.

Corollary 2. When the inclination of the planes MO, NO, is leſs than a given angle, the ear cannot diſtinguiſh the relative poſitions of the ſounding bodies; it therefore refers them to the ſame place.

The firſt time I peruſed Dr. Smith's Harmonics, Dr. Young's objection occurred to me; but the preceding train of arguments removed the ſcruple, without diſcovering the author's reaſons for treating this article of his work with ſo much brevity. Perhaps the demonſtration, which coſt me an effort of ſtudy, was an intuitive concluſion in his comprehenſive mind. As ſoon as the propoſition was eſtabliſhed, I aſſented to his definition of an interval of ſound, allowing it to be a quantity of a certain kind, terminated by a graver and an acuter ſound. The demonſtration of Prop. V. convinced me, that intervals of this ſort may be ſubdivided by the interpoſition of one or more intermediate ſounds, which conceſſion formed the baſis of my analyſis of the human voice.* Speculative men may differ in opinion about the origin of the ſmall intervals which form the tones of various voices; but they muſt exiſt, whether we aſcribe them to an undulating motion like that of a ſtretched cord, or to the cotemporary vibrations of a ſyſtem of elaſtic bodies. It does not appear, that Dr. Young was acquainted with my paper at the time he compoſed his own; but he found it neceſſary to allow the tone of the larynx to receive various modifications from the vibrations of the adjacent parts. His theory therefore differs from mine in this particular only: he pronounces the voice to be a compound by coaleſcence; I deny the poſſiblility of ſuch a compound, and call it a mixture of imperfect uniſons. This mixture appears to be a ſingle ſound, becauſe it has but one direction; for the proximity of the various parts contributing to the formation of it, diſqualifies the ear, ſo that it cannot perceive their relative poſitions, and compels it to refer them all to one place, by Corollary 2 to propoſition V.

Interval of ſound.

Analyſis of the human voice.

* Mancheſter Mem. v. 58, or Phil. Journal, Quarto, IV. 46. for an abridged ſtatement.

A certain

A certain clafs of founds,* which, for the fake of brevity, were not noticed in my paper on the voice, deferve a place in the prefent communication. If a finely-toothed file pafs flowly over a fmooth elaftic fubftance, fuch as a piece of horn, it makes a grating noife; but if the velocity of the inftrument be fufficiently increafed, a continued found is produced, which becomes more or lefs acute, by giving a quicker or flower motion to the file. The grating noife is occafioned by a fucceffion of fhort interrupted founds, refulting from the united vibrations of the file and the body it fcratches; but the quick fucceffion of thefe founds, caufed by an increafe of velocity, gives rife to a fecondary found refembling the harmonical notes, being produced by a like caufe. Now this found becomes a primary object with the ear, in all probability becaufe the pitch of it may be varied; for the firft founds proceeding from the action of the file, evidently fupply nothing but the tone. Many inftances of the kind occur in art and nature: the notes of all reed-inftruments are of this defcription, and the voice muft be referred to the fame clafs, becaufe the larynx refembles a reed-inftrument in ftructure.

Clafs of founds produced by the quick fucceffion of other founds; the firft giving the tune or pitch, and the latter the tone or character.

VI.

Experiments and Obfervations to determine whether the Quantity of Rain and Dew is equal to the Quantity of Water carried off by the Rivers and raifed by Evaporation; with an Enquiry into the Origin of Springs. By Mr. JOHN DALTON *.

IT is fcarcely poffible to contemplate without admiration the beautiful fyftem of nature by which the furface of the earth is continually fupplied with water, and that unceafing circulation of a fluid fo effentially neceffary to the very being of the animal and vegetable kingdoms takes place. Naturalifts, however, are not unanimous in their opinions whether the rain that falls is fufficient to fupply the demands of fprings and rivers, and to afford the earth befides fuch a large portion for evaporation as it is well known is raifed daily. To afcertain this point is an object of importance to the fcience of agriculture, and to every concern in which the procuration and

Interefting fyftem by which the globe is fupplied with water.

Whether the rain be equal to all the demands of rivers, &c.

* Manchefter Memoirs, V. 346.

management

management of water makes a part, whether for domeſtic purpoſes or for the arts and manufactures.

For the ſake of perſpicuity I have diſtributed the ſubject under four heads:

Diviſion of the ſubject.

1. Of the quantity of rain and dew.
2. Of the quantity of water that flows into the ſea.
3. Of the quantity of water raiſed by evaporation.
4. of the origin of ſprings.

SECTION I.

An Eſtimate of the Quantity of Rain and Dew that falls in England and Wales in a Year.

Annual quantity of rain and dew in England and Wales. Inland counties have leſs rain than coaſts; and mountainous diſtricts have moſt.

Rain-gages have been fixed of late years in almoſt every part of the kingdom; by means of them we are enabled to determine, with conſiderable exactneſs, the depth of water that the rain yields in any given place. Inland counties have leſs rain than maritime ones, eſpecially thoſe which border on the weſtern ſeas. But a ſtill greater difference ſeems to take place between a mountainous country and a champaigne, or flat country: in the former there often falls double or triple the quantity of rain in a year, that there does in the latter, and never leſs than an equal quantity. It may be obſerved, that ſeveral years account of the rain at any place is reqnired before a medium yearly quantity can be obtained with ſufficient accuracy. The following is perhaps the largeſt collection of accounts of rain fallen in different places in England that has hitherto appeared: They are moſtly taken from the Tranſactions of the Royal and other Societies.

Quantities tabulated.

Counties (maritime)	*Places.*	Mean annual depth in inches.
CUMBERLAND - -	Keſwick, 7 years - -	67. 5
	Carliſle, 1 year - - -	20. 2
WESTMORLAND -	Kendal, 11 years - - -	59. 8
	Fell-foot, 3 years - -	55. 7
	Waith Sutton, 5 years -	46
LANCASHIRE - -	Lancaſter, 10 years - -	45
	Liverpool, 18 years - -	34. 4
	Mancheſter, 9 years - -	33
	Townley - - -	41
	Crawſhawbooth, near Haſlingden, 2 years - - -	60

GLOUCESTERSHIRE

Counties (maritime)	*Places.*	Mean annual depth in inches.
Gloucestershire	Briftol, 3 years - -	29. 2
Somersetshire -	Bridgewater, 3 years - -	29. 3
Cornwall - - -	Ludguan, near Mount's Bay, 5 years - - - - - - -	41
	Another place, 1 year - -	29. 9
Devonshire - -	Plymouth, 2 years - - -	46. 5
Hampshire - - -	Selbourne, 9 years - - -	37. 2
	Fyfield, 7 years - - - -	25. 9
Kent - - - - - -	Dover, 5 years - - - -	37. 5
Essex - - - -	Upminfter, - - - - - - -	19. 5
Norfolk - - - -	Norwich, 13 years - - -	25. 5
Yorkshire - - -	Barrowby, near Leeds, 6 years	27. 5
	Garfdale, near Sedbergh, 3 y.	52. 3
Northumberland	Widdrington, 1 year - - -	21. 2
Counties (inland)	*Places.*	*Means.*
Middlesex - - -	London, 7 years - - - -	23
Surrey - - - -	South Lambeth, 9 years - -	22. 7
Hertfordshire -	Near Ware, 5 years - -	25
Huntingdonshire	Kimbolton, 7 years - - -	25
Derbyshire - -	Chatfworth, 15 years - -	27. 8
Rutlandshire -	Lyndon, 21 years - - - -	24. 3
Northamptonshire	Near Oundle, 14 years - -	23
	General Mean - -	35. 2

This general mean of 35. 2 inches is, I apprehend, a little above the medium for England and Wales, as the greater number of places are thofe where much rain falls. If we take a mean for each of the above-mentioned counties (where more than one place in a county is given) and then a general mean from the counties, the refult is a reduced mean of 31. 3. Even then it may be objected that the greater part of the counties are maritime; but it muft be obferved, that there is no account of rain in Wales; and we may fafely conclude, that the rain in Wales would exceed the laft-mentioned mean as Mean quantity of rain.

 much

much as the inland counties of England, not in the above lift, would fall fhort; becaufe Wales is both a mountainous country, and expofed to the fea.

Corrected is 31 inches for England and Wales.

We will therefore conclude, that the mean annual depth of rain in England and Wales, deduced from thefe 20 counties, is 31 inches: A quantity which fubfequent obfervations, I am confident, will not diminifh, and probably not increafe much *.

Quantity of dew. It is water depofited by the cold of night.

It remains to eftimate the quantity of dew that falls in a year. Some have doubted whether dew is derived from the air or the earth; but a proper attention to the phenomena will fatisfy us, that it is a depofition of water, evaporated during the heat of the day. With refpect to the quantity that falls in a year, we are much at a lofs, as no daily obfervations have been made for a feries of time that I know of: indeed, it would be difficult to prefcribe a mode of obfervation. Dr. Hales † relates fome experiments made to determine the quantity of dew that falls upon moift earth, from which he eftimates the annual dew at 3. 28 inches. But it is probable that the dew which is depofited on grafs is much more copious than what falls on moift earth, becaufe grafs expofes much more furface in a given acre of ground. If we take the dew at five inches annually, it will probably not be much over-rated: fuppofing it fhould be over-rated, the excefs may ftand againft the rain that is loft by evaporation from the furface of the rain-gage each time it rains ‡. Wherefore, upon the whole, we fhall

Eftimate by Dr. Hales.

Annual quantity of dew taken at five inches.

* The editors of the Encyclopedia, under the article Weather, from 16 places of obfervation, make the annual mean for Great Britain 32. 53 inches; and M. Cotte, in the Journal de Phyfique for 1791, gives a mean derived from 147 places in different parts of the world equal to 34. 7 inches.

† Veg. Statics, Vol. I. page 52.

‡ Since writing the above paragraph on dew, I have had occafion to make feveral experiments on the fubject of aqueous vapour, as it exifts in the atmofphere, the refult of which will, I am perfuaded, materially illuftrate this important queftion in phyfics.—At prefent I fhall only obferve, that the following conclufions feem deducible from the experiments above referred to.

Generalities concerning aqueous vapor.

1. That aqueous vapour is an elaftic fluid *fui generis*, diffufible in the atmofphere, but forming no chemical combination with it.

2. That temperature alone limits the *maximum* of vapour in the atmofphere.

3. That

ſhall have 36 inches of water at a medium annually on the ſurface of the earth in England and Wales, reckoning 31 for rain and five for dew. Annual quantity of rain and dew 36 inches.

According to Guthrie, the area of England and Wales is 46.450 ſquare miles. This reduced to ſquare feet, gives 1.378.586.880.000: which, multiplied by three feet the annual depth of rain and dew, gives 4.135.760.690.000 cubic feet of water = 153.476.320.000 cubic yards, or 28 cubic miles = 115 thouſand millions of tons in weight, nearly. We muſt now conſider how this enormous quantity of water is diſpoſed of. Computed quantity of water that falls in a year 28 cubic miles, or 115 thouſand million of tons.

There are two principal ways by which the water derived from rain is carried off again: one part of it runs off immediately into rivulets, or ſinks into the earth a ſmall way, breaks out again in lower ground in the form of ſprings, thence makes its way to ſome river, by which it is conveyed into the ſea—another part is raiſed into the atmoſphere by evaporation. We take no notice here of the decompoſition of water by vegetables; becauſe it is preſumed that in the courſe of nature the principles are combined and water formed again. It is carried off by rivers, by ſprings, and by evaporation.

3. That there exiſts at all times, and in all places, a quantity of aqueous vapour in the atmoſphere, variable according to circumſtances.

4. That whatever quantity of aqueous vapour may exiſt in the atmoſphere at any time, a certain temperature may be found, below which a portion of that vapour would unavoidably fall or be depoſited in the form of rain or dew, but above which no ſuch diminution could take place, chemical agency apart. This point may be called the *extreme temperature* of vapour of that denſity.

5. And that whenever any body colder than the extreme temperature of the exiſting vapour is ſituated in the atmoſphere, dew is depoſited upon it, the quantity of which varies as the ſurface of the body and the degree of cold below the extreme temperature.

N. B. The *extreme temperature* of vapour in the atmoſphere varies all the way from the *actual* temperature of the atmoſphere to 10, 15, 20 or more degrees below it.—The point may generally be found in the hotteſt months by pouring cold ſpring water into a dry and clean glaſs, and marking what degree of cold is ſufficient to produce a dew on the outſide of the glaſs; at other times frigorific ſaline ſolutions may be uſed.

SECTION 2.

An Eſtimate of the Quantity of Water that flows into the Sea from England and Wales in a Year.

Method of determining how much water flows through a river.

To calculate the quantity of water that flows down any one river into the ſea in a given time, ſeems at firſt view a queſtion of great difficulty. The neceſſary data, however, may be obtained with conſiderable exactneſs, by proper obſervations, and then it becomes an eaſy caſe of menſuration. Dr. Hutton, in his Philos. and Mathemat. Dictionary, article River, propoſes a very good method to determine by experiment the velocity of a river:—A cylindrical piece of light wood, its length ſomewhat leſs than the depth of the waters, is to be taken, and a few ſmall weights attached to one end in order to make it ſwim upright. To the other end a ſmall rod is fixed in the centre in direction of the axis. This being ſuffered to float down the ſtream will move with the velocity of the water; and if the rod be obſerved to incline towards the river upward or downward, it ſhews the current to be more rapid at the bottom or ſurface reſpectively.

This experiment being made in the middle and near the ſides of a river, a medium velocity may be obtained. Then the medium, breadth, depth, and ſpace run over in a certain time being multiplied together, will give the quantity of water that flows down in that time.

Eſtimate of water delivered by the Thames.

Dr. Halley, in order to eſtimate the quantity of water that flows into the Mediterranean ſea by means of rivers, makes a compariſon of the great rivers of Italy, &c. with that of the Thames. (Philos. Tranſact. Abridg. Vol. 2. Page 110). He aſſumes the breadth of the Thames at Kingſton Bridge to be 100 yards, its depth three yards, and velocity two miles per hour. He profeſſedly over-rates the dimenſions, in order to allow more than a ſufficiency for the ſtreams received below Kingſton. This aſſumption gives the area of a tranſverſe ſection of the river = 300 ſquare yards, and the quantity of water flowing down = 20.300 000 tons in a day. This muſt be over-rated by at leaſt, I think, one third:—If the breadth be aſſumed 100 yards, the depth three, and velocity two miles per hour, it will then give $\frac{2}{3}$ of the reſult above mentioned; or it will amount to the ſame thing if we take $\frac{1}{8}$ part from all the three data aſſumed by Dr. Halley, the reſult being $\frac{2}{3}$ of that above,

above, amounting in the year to 166.624.128.000 cubic feet, which is a little more than $\frac{1}{25}$ part of all the rain and dew in England and Wales in a year, as above deduced.

By an infpection of the annexed map of the rivers of this country, as well as by a fair calculation, it appears, that the water of the Thames is drawn from an extent of country of about 600 fquare miles, or $\frac{1}{8}$ of the area of the whole, nearly. The Severn, including the Wye, fpreads over an equal or greater extent of country: and that collection of rivers which conftitutes the Humber is fuperior to either of the other two in this refpect. As far as my own obfervation goes, the Severn and Wye muft difembogue as much or more water than the Thames; the Humber I have not feen collectedly, but have noticed moft of the branches conftituting it, and fhould apprehend it cannot be inferior to the Thames: all other circumftances being the fame, the quantity of water carried down by any river fhould be as the area of the ground from which the water is derived, and on this account the Humber ought to exceed the Thames *.

View of all the rivers of England and Wales,

The Severn, which is partly derived from the mountainous country of Wales, is certainly the moft rapid of the three rivers, and probably carries down the moft water: as the Thames, however, is generally confidered to take the lead, we will fuppofe, upon the whole, that thefe three rivers are equal in this refpect.

and the diftricts

The counties of Kent, Suffex, Hampfhire, Dorfetfhire, Devonfhire, Cornwall, and Somerfetfhire, from the Medway to the lower Avon inclufively, in an extent of 11.000 fquare miles, do not prefent us with many large rivers. From their nnmber and magnitude, we cannot form a high eftimate of their produce. The quantity of rain for thofe counties is indeed near the average for the kingdom, as far as the preceding obfervations determine; but the milder temperature of their winters and greater heat of their fprings and fummers, will caufe a greater evaporation than in fome other parts: It is probable the rivers in thefe counties may amount, when taken together, to $1\frac{1}{2}$ times the magnitude of the Thames. The rivers that difembogue their waters on the coaft of Lincolnfhire,

which fupply them with water.

* A more perfect theorem will be given afterwards, for finding the quantity of water carried down by any river.

colnſhire, Norfolk, Suffolk and Eſſex, from the Humber to the Thames, though drawn from a country of 7000 ſquare miles, manifeſtly fall far ſhort of the Thames. The two places in this diſtrict, for which we have accounts of the rain, Norwich and Upminſter. give a mean of only 22½ inches annually. This, with the flatneſs of the country, which prevents the water from running off in ſome degree, makes the rivers much leſs than what might otherwiſe be expected from the extent of ground. There are but three or four of any conſequence, Probably all the rivers may amount to half the ſize of the Thames. There remains above 6000 ſquare miles in Wales, from the Wye to the Dee, incluſive of the laſt, and the northern counties of Lancaſter, Weſtmoreland, Cumberland, Northumberland, and Durham, with part of Cheſhire and a ſmall part of Yorkſhire, from the Merſey round by the Tweed to the Tees, amounting to 7 or 8000 ſquare miles, to be eſtimated.

The rivers upon the whole are conſidered as carrying off 13 inches out of the 36 in their fall annually.

Theſe two diviſions, though not larger than ſome others, abound in rivers, many of which are conſiderable in magnitude and of great rapidity. The rains at an average, it is probable, are double what they are in the S. E. counties of the kingdom. The rivers in theſe two diſtricts cannot fairly be eſtimated, I think, at leſs than *four* times the Thames. It appears, then, that by this eſtimation, the water carried off by all the rivers in England and Wales, may amount to *nine* times that carried off by the Thames = 13 inches of rain. There remains ſtill *ſixteen* times the water of the Thames, or 23 inches of rain to account for, before we have diſpoſed of all the rain and dew.

SECTION 3.

An Eſtimate of the Quantity of Water raiſed by Evaporation.

Evaporation

Upon looking over the ſurface of any country, three principal varieties of ſurface preſent themſelves to view, as far as reſpects evaporation, namely, *water*, ground covered with graſs and other vegetables, and bare ſoil. The difficulties that occur in attempts to find the quantity of water evaporated in thoſe three caſes, are perhaps the principal reaſon why our knowledge on this head is ſo imperfect.

from water, bare ſoil, and covered ground.

As far as experiments hitherto made authoriſe us to draw concluſions, it ſhould ſeem that the evaporation from water is greateſt;

greateſt; that from green ground is probably next, and that from bare ſoil the leaſt: though we may preſume, that the copious dews upon the graſs more than ſupply the exceſs of evaporation above what takes place from a moiſt uncovered ſoil.

The moſt ſatisfactory experiments I have ſeen an account of, relating to the evaporation from a ſurface of water, are thoſe of Dr. Dobſon, made at Liverpool, in the years 1772, 73, 74 and 75. (Vid. Philos. Tranſac. Vol. 67.)—He took a cylindrical veſſel of 12 inches diameter, and having nearly filled it with water, expoſed it beſides his rain-gage of the ſame aperture, and by adding water to it, or taking it away occaſionally, he kept the ſurface nearly of the ſame height, and carefully regiſtered the quantities added or taken away. by a compariſon of which with the rain, the amount of the evaporation was aſcertained. The mean monthly evaporation for four years was—January 1.50 inches.—February 1.77. —March 2.64.—April 3.30.—May 4.34.—June 4.41.—July 5.11.—Auguſt 5.01.—September 3.18.—October 2.51.—November 1.51.—December 1.49.—In all 36.78 inches. The mean rain for the ſame time was 37.48 inches.—In the year 1793 I found the evaporation from water in a ſimilar way at Kendal for 82 days in March, April, May and June to be 5.414 inches. The greateſt quantity evaporated on one of the hotteſt and drieſt days in ſummer was a little above ,2 of an inch in depth. Dr. Dobſon's experiments on evaporation from water.

The experiments to determine how much is evaporated from green ground and from moiſt earth, are very few that have come to my knowledge. Dr. Hales, from a few experiments, calculates that moiſt earth only throws off $6\frac{2}{3}$ inches annually.—This calculation muſt be far below the truth. Dr. Watſon, Biſhop of Llandaff, found that in a dry ſeaſon there evaporated from a graſs plat that had been mowed cloſe, about 1600 gallons in an acre per day, which amounts nearly to ,07 of an inch in depth; and that after rain the evaporation was conſiderably more. Now ſuppoſing ,07 to be the medium daily evaporation for May, June, July and Auguſt, and that as much is raiſed in theſe four months as in all the reſt of the year, the annual evaporation in ſuch circumſtances will be 17 or 18 inches, which is but half that obſerved from water at Liverpool, and ſix inches leſs than the reſerve of rain ſtated above. Hales's eſtimate from moiſt earth. Dr. Watſon from a graſs plat.

In

Experiment on a tube of earth to ſhew how much water ran off the ground; how much ſunk in; and how much evaporated.

In order to aſcertain this point more fully, and to inveſtigate the origin of ſprings, my friend Thomas Hoyle, jun. and ſelf, practiſed an expedient as follows, beginning in the autumn of 1795. Having got a cylindrical veſſel of tinned iron, 10 inches in diameter and three feet deep, there were inſerted into it two pipes turned downwards for the water to run off into bottles: the one pipe was near the bottom of the veſſel; the other was an inch from the top. The veſſel was filled up for a few inches with gravel and ſand, and all the reſt with good freſh ſoil. It was then put into a hole in the ground, and the ſpace around filled up with earth, except on one ſide, for the convenience of putting bottles to the two pipes; then ſome water was poured on to ſadden the earth, and as much of it as would was ſuffered to run through without notice, by which the earth might be conſidered as ſaturated with water. For ſome weeks the ſoil was kept above the level of the upper pipe, but latterly it was conſtantly a little below it, which precluded any water running off through it. Moreover, for the firſt year the ſoil at top was bare; but for the two laſt years it was covered with graſs the ſame as any green field. Things being thus circumſtanced, a regular regiſter has been kept of the quantity of rain water that ran off from the ſurface of the earth through the upper pipe (whilſt that took place) and alſo of the quantity of that which ſunk down through the three feet of earth, and ran out through the lower pipe. A rain-gage of the ſame diameter was kept cloſe by to find the quantity of rain for any correſponding time.

Th

The following Table ſhews the Reſult. Tabulated reſults.

	Water through the two Pipes.			Mean.	Mean Rain.	Mean Evap.
	Inch. 1796.	Inch. 1797.	Inch. 1798.	inch.	inch.	inch.
Jan.	1.897—	,680—	1.774+	1.450+	2.458	1.008
Feb.	1.778—	,918—	1.122	1.273	1.801	,528
March	,431—	,070—	335	,279	,902	,623
April	,220—	,295—	,180	,232	1.717	1.485
May	2.027—	2,443+	,010	1.493+	4.177	2.684
June	,171—	,726—	——	,299	2.483	2.184
July	,153—	,025	——	,059	4.154	4,095
Aug.	——	——	,504	,168	3.554	3.386
Sept.	——	,976	——	,325	3.279	2.954
Oct.	——	,680	——	,227	2.899	2.672
Nov.	——	1.044	1.591	,879	2.934	2.055
Dec.	,200	3.077	1,878+	1.718+	3.202	1.484
	6.377—	10.934—	7,379	8.402	33.560	25.158
Rain	30 629—	38.791—	31,259			
Evap.	23.725—	27.857—	23.862			

The following obſervations were made when the water paſſed through both pipes: that is, when the veſſel was filled up with earth above the level of the upper pipe. Variation of the experiments.

		Top pipe Inch.	Bottom pipe. Inch.
1796. Jan.	25	,190	,280
	30	,080	,114
Feb.	2	,100	,254
	8	,196	,140
May	1	,163	,000
	10	,060	,400
	12	,312	,175
	15	,190	,200
June	3	,120	,040
	Total	1.411	1.603

The column of mean evaporation is derived by taking the difference of the two columns preceding it; but it ſhould be obſerved that though this method is ſufficiently exact in taking the Remarks on the tables.

the year together, it is not ſo in taking the months ſeverally, becauſe it preſumes that the earth in the veſſel contains the ſame quantity of water at the end of each month, or is ſaturated with it; whereas in the ſummer months it is frequently ſhort of ſaturation. The conſequence is, that the evaporation appears from this table to be ſomething leſs than it really is in the ſummer months, and ſomething more in the antumnal *.

Concluſions. Evapor. from ground is 30 inches.

From theſe experiments it ſeems we may conclude—1ſt. That the quantity of water evaporated, *in the circumſtances above related,* amounts to 25 inches of rain annually; to which if we add five inches for the dew, it will give 30 inches of water raiſed annually.

It increaſes with the rain.

2d. That the quantity of evaporation increaſes with the rain, but not proportionally. Thus, 1797 gave the moſt rain and the greateſt evaporation, &c.

Deep bare ſoil and ſod do not differ.

3d. That it does not appear there is much difference betwixt the evaporation from bare earth, when there is ſufficient depth of ſoil, and that from ground covered with vegetating graſs. The account in 1796 is much what might have been expected, if the earth had been covered with graſs.

Whether this evap. of 30 and the rivers of 13, making 43 inſtead of 36 (the rain) indicate another ſupply?

As this account of evaporation, amounting to 30 inches, exceeds the medium reſerve of rain of 23 inches, it demands an enquiry whether the rain is adequate, or whether the earth derives a ſupply of water from ſome ſubterranean reſervoir, according to the opinion of ſome philoſophers.

Reply; negative.

With reſpect to the deficiency of 7 inches, there are *three* cauſes to be aſſigned for it, which appear to me fully ſufficient, without having recourſe to any ſource but that of rain for the ſupply of the earth in general.

The evapor. was taken a little too great.

1ſt. In the account of the rain that paſſed through the earth in our evaporating veſſel, there are a few monthly products marked, + thoſe were occaſioned by the bottle that received the water through the pipe being found with the water running over; this loſs was placed to the account of evaporation; it could not be much, as the water was taken ſeveral times in a month, but poſſibly might amount to one inch in the year.

The rain at Mancheſter exceeds the medium,

2d. The rain at Mancheſter, being 33½ inches annually, exceeds the medium of 31 inches; and conſequently, ac-

* N. B. The earth in the veſſel always appeared as well ſupplied with moiſture as the ground around it, in the drieſt weather.

cording

cording to the preceding obfervations, the evaporation ought to exceed the medium.

3d. But the principal caufe of the excefs in our account of evaporation, I conceive to be the prevention of the water running off from the furface of the earth at the top, by having the earth below the level of the upper pipe: it has been feen, that when the earth was above that level, a great part of the water came off that way, by which the furface was fooner dried: whereas by forcing all the water to fink through the earth or ftand on its furface, a greater degree of moifture perpetually exifted at the furface, and confequently afforded a greater fcope for evaporation, than the furface of the earth in general would do.

and the water not being fuffered to run freely off at top, did increafe the evaporation.

Upon the whole then I think we may fairly conclude—that the *rain* and *dew* of this country are equivalent to the quantity of water carried off by evaporation and by the rivers. And as nature acts upon general laws, we ought to infer, that it muft be the cafe in every other country, till the contrary is proved.

Hence the rain and dew fully fupply the evaporation and the rivers.

This conclufion being admitted, we are enabled to deduce a general theorem for the quantity of water carried down into the fea by any river in any country (on the fuppofition that all rivers are ramified alike) provided we have certain *data*: thefe *data* are the length of the river, and the excefs of the rain above the evaporation in the country from which the water of the river is drawn: alfo, it fhould be known by obfervation, how much water fome one given river carries down.

Theorem for deducing the quantity of water carried by a river, &c.

For, from the principles of geometry, the area of country from which any river is fupplied, will be as the fquare of the length of the river; and the quantity of water carried off, will be in the compound ratio of the area of the country, and the excefs of the rain and dew above the evaporation.

Thus, let $L =$ the length of any river, $E =$ the excefs of rain and dew above the evaporation, and $Q =$ the quantity of water difembogued in any given time by that river; $l =$ the length of any other river, $e =$ the excefs, &c. and $q =$ the quantity of water; then we fhall have $q = \frac{Q\,l\,e}{L\,E}$.

Statement.

Ex. gr. Suppofe the length of the Thames = 200 miles, and the excefs = 5 inches, eftimating the rain and dew at 30 inches and evaporation at 25; and fuppofe the river Kent, in

Example.

Weftmorland,

Weftmorland, to be 20 miles in length, and the excefs 35 inches, the rain and dew being fuppofed 65, and evaporation 30 inches.

$$\text{Then, } \frac{20^2 \times 35 \times Q}{200^2 \times 5} = \frac{7\,Q}{100} = q \text{ or } Q = 14\tfrac{2}{7}\,q\text{; which}$$

refult, I believe, will be found to accord nearly with the meafurement of the two rivers on the principle before mentioned.

SECTION 4.

On the Origin of Springs.

Caufe of fprings. The origin of fprings has always been juftly confidered as a queftion of natural hiftory worthy of inveftigation.—In the infancy of fcience hypothefes are formed to account for phenomena; but when facts are difcovered totally inconfiftent with an hypothefis, it ought to be difcarded. This does not feem to have been the cafe in the fubject before us; for various opinions are ftill held by fome, which it is impoffible to fupport by facts. The object of the following remarks and experiments is to afcertain the difputed point if poffible.

Opinions. There are *three* opinions refpecting the origin of fprings which it may be proper to notice.

1. Rain and dew. 1ft. That they are fupplied entirely by rain and dew.

2. Subterranean refervoirs. 2d. That they are principally fupplied by large fubterranean refervoirs of wates.

3. Filtration from the fea. 3d. That they derive their water originally from the fea, on the principle of filtration.

Whether the firft caufe be infufficient. It is obvious, that before we pay any attention to the two latter opinions, the caufes affigned in the firft ought to be proved infufficient by direct experiment. M. de la Hire is the only one who has attempted to do this, as far as my information extends, in the Parifian Memoirs for 1703. He procured a leaden veffel eight feet deep, having a pipe at the bottom; this he buried in the earth, and filled with foil of fand and loam, expofing the furface to receive all the rain that fell. After 15 years trial, he found that no water had run through the pipe at the bottom.

De la Hire's experiment. No rain foaked through a mafs of earth eight feet thick.

Shallower veffel. Again, he took another veffel, eight inches deep, which he filled with earth and expofed in like manner. No rain penetrated fo as to run out at the bottom from June to February;

but after that time it yielded a quantity after moſt rains. Another veſſel of twice the depth, or 16 inches, gave a reſult much like that of eight inches. Farther, M. de la Hire found, that when herbs were planted in the ſoil of the laſt mentioned veſſel, and grown up, no rain penetrated through the ſoil, but inſtead thereof it was not ſufficient to ſuſtain the vegetation; for the plants would require to be ſprinkled occaſionally, or elſe they began to droop and wither.

With reſpect to the firſt mentioned fact, we need not wonder that no water penetrated through eight feet of earth at Paris, where the annuul rain is but 20 inches, when only eight or nine inches penetrated through three feet of earth here, where the rain is 33 or 34 inches annually. But it does not follow that rain may not deſcend down declivities of the ground into vallies or lower parts, at Paris as well as here, and being accumulated may penetrate into the earth to a conſiderable depth, eſpecially if it meet with channels or chaſms of any kind, or declining ſtrata of earth that are impenetrable by water. Paris, I believe, however, is not very liberally ſupplied with ſprings, as might be expected. As to the experiment upon vegetation, it only proves that the rain in ſpring and ſummer is ſometimes not ſufficient to ſupport vegetable life, a fact which may readily be granted; but then in his experiment the plants were precluded from a ſupply of moiſture from the earth beneath the veſſel, which is a reſerve of the utmoſt conſequence in dry ſeaſons.

Remarks. Paris has only 20 inches annual rain; but the higher grounds there as well as here may afford waters, &c.

This circumſtance of water aſcending again in the earth, on whatever principle it is effected, cannot be denied.—There were 4¾ inches of rain here in July laſt, none of which paſſed through the earth in the evaporating veſſel; this earth, however, at the end of the month, was far from that degree of dryneſs which is unfit for the ſupport of vegetation.—During the firſt four days of Auguſt there fell about three inches of rain, and only ½ an inch penetrated through the earth in the evaporating veſſel. Conſequently three feet in depth of earth that was moderately moiſt imbibed nearly three inches of rain before it was ſaturated; whence we may conclude that three inches nearly had aſcended and been evaporated. This evidently ſhews, that earth is capable of holding a very great proportion of water, that in ſummer the water aſcends to ſup-

Water after ſoaking into the earth aſcends again.

ply

ply the exigencies at the furface, and that earth far under the point of faturation with moifture is ftill fit to fupport vegetation.

Queftion. How much water is contained in faturated ground.

This obfervation fuggefted the following queftion—How much water is there in a given depth of earth when the foil is at the point of faturation, or in that ftate when it begins to yield water from the lower pipe of the evaporating gage?

Experiment. 1 foot faturated earth contains 7 inches water, and it may lofe half before it is too dry for vegetation.

To determine this I took a quantity of garden foil that had been foaked with rain a day before, and preffed it into a crucible; in this ftate I found its fpecific gravity to that of water as five to three. It was then expofed to a moderate heat till it appeared, as near as I could judge, of the fame moifture as garden foil two inches deep in dry fummer weather; afterwards it was expofed almoft to a red heat till it became a perfectly dry powder; in the former cafe it loft $\frac{1}{12}$ of its weight, and in the latter $\frac{1}{3}$.—When it had loft $\frac{1}{6}$, it did not appear too dry to fupport vegetation. When it had loft $\frac{2}{9}$, it appeared like the top foil in fummer.—Hence it follows, that every foot of earth in depth, fo faturated, contains feven inches of water, and that it may part with one quarter of its water, or even one half, and not be too dry for fupporting vegetation.

Brick clay nearly the fame.

Clay, juft dug out for the purpofe of making bricks, was tried in the fame manner: It gave the fame fpecific gravity as the earth, and yielded not much lefs water.

De la Hire's conclufions erroneous.

Thefe experiments and obfervations prove, that M. de la Hire's conclufions, drawn from the vegetation of plants in a given quantity of foil, precluded from any communication with the earth at large, are erroneous, or at leaft unwarranted: As it does not thence appear that the evaporation for the *whole* year exceeds the rain in the year, whatever it may do for a month or two in fummer.

Springs are therefore fupplied by the rains.

The origin of fprings may ftill therefore be attributed to rain, till fome more decifive experiments appear to the contrary; and it becomes unneceffary to controvert the other two opinions refpecting this fubject.

Springs are thence deficient in fummer.

Upon the whole it fhould feem, that at the commencement of fpring, the ground is nearly faturated with water for five or fix feet in depth, as the rains and dews in autumn and winter far exceed the evaporation: There are then five or fix inches of water at leaft to be raifed up again to the furface in

cafe

cafe of exigence in the fpring and fummer: If this happen to be fo, then it is at the expence of fprings; for we find the generality of fprings become languid, or entirely ceafe to flow at the end of a long drought. As to the few fprings that feem to be little affected by dry or wet feafons, they form exceptions which it would not be difficult to account for.

VII.

Defcription of an Inftrument for extracting Hard Subftances which may ftick during their Paffage to the Stomach. By G. C.

To Mr. NICHOLSON.

SIR,

ENCLOSED, I fend you a drawing of an untried inftrument; if you think it likely to accomplifh the end for which it was defigned, you will, perhaps, give it a place in your very ufeful work. This inftrument, I conceive to be an improvement of that commonly ufed for forcing down any hard fubftances that may ftick between the mouth and the ftomach. In many cafes nails, pins, and other metallic matters, get into this fituation, when it would undoubtedly be preferable to draw them up through the mouth, inftead of paffing them into the ftomach, where they are no fooner arrived, than they furnifh a new fpecies of danger to the fufferer. With this view the following inftrument was conftructed. A, B, Pl. XI. is a rod of whalebone, having a fmall groove down the middle, from end to end, large enough to contain a ftrong filken thread; this thread is confined to the groove by a few lappings of fine waxed filk. At B, is faftened, as ufual, a fponge, about one third of the common fize. Juft above the fponge is fixed a fmall pulley, round which the filken thread winds, and returning up the oppofite fide of the whalebone to that on which it defcended, is tied faft to the bottom of a fmall leather cap C. Above this cap, at D, are faftened 12 or 14 fmall filver wires, made to fpring into the form reprefented in Fig. 2. Thefe wires by means of loops at their ends, fupport a round bag of net-work of fine filk, perforated in the centre to admit the whalebone rod. Thefe wires, together with the bag, muft be capable of being inferted and confined in the cap C,

Inftrument for extracting bodies from the throat.

Defcription.

fo

ſo as to remain in the poſition repreſented in Fig. 1. in which ſtate it is ready for being paſſed down the oeſophagus; and it ſeems ſcarcely neceſſary to remark, that previous to the inſtrument being withdrawn, the cap C muſt be pulled off, by means of the thread at A; when the wires will expand the net, and preſs its edge cloſe to the gullet on every ſide, and in returning to the mouth, will probably bring up any ſubſtance that was lodged there, within it.

G. C.

Brompton, Jan. 23, 1803.

VIII.

On the Flexure of Wax and other Bodies by irregular cooling, with Conſiderations on the Probability that it may be cauſed by the Law of Cryſtallization. In a Letter from R. B.

To Mr. NICHOLSON.

London, Feb. 16, 1803.

SIR,

Thread of wax flowing down a candle,

A STREAM of wax has juſt overflowed the cup of the wax candle by which I have been reading, and has preſented me with a fact or two which I think worthy the meditation of philoſophers. If you think ſo, pleaſe to give them a place in your repoſitory.

becomes ſeparated by ſpontaneous flexure.

The fluid wax has formed a line or protuberance on the outſide of the candle, four inches in length, a little more than one fifteenth of an inch in width, or ſurface applied to the candle, and one tenth of an inch in elevation or thickneſs. As it grew cold it has ſeparated from the candle, ſo that its lower extremity ſtands a little more than half an inch diſtant, and it does not touch for the length of two inches and a half from the bottom; all the upper part ſtill continuing adherent. And laſtly, the ſeparated portion exhibits a regular curve, convex towards the candle, and more convex the nearer the lower extremity; ſo that, when carefully taken off, and applied to ſeveral circular arcs deſcribed upon paper, the difference was very obviouſly perceived.

The wax was cooled by different parts in ſucceſſion.

Upon theſe facts I obſerve from obvious reaſons that the wax which flowed in contact with the ſolid candle, was more ſpeedily

ſpeedily cooled than that which flowed on the outſide of that heated wax; and that that portion of the wax which flowed in contact with the candle, and ran to the greateſt diſtance was the moſt ſpeedily cooled.

And the reſult farther ſhews, 1. That the dimenſions of wax ſuddenly cooled, are larger than thoſe of wax cooled more ſlowly; and 2. The quicker the cooling the greater this difference. For the fluid wax was depoſited in a ſtrait line, and its curve figure after cooling ſhewed that the interior line or convex limit was longer then the exterior or concave limit; and this difference being greateſt where the refrigeration was moſt ſudden, namely, towards the lower extremity, was ſhewn in the greater curvature.

Deduction. Wax quickly cooled is leſs denſe than if ſlowly cooled.

I will not, Sir, do your readers the injuſtice to ſuppoſe any of them will think the dignity of philoſophy impaired by a ſpeculation on the guttering of a candle, but will proceed in my diſquiſition in hopes that others of more leiſure and ability may purſue the object farther, if found to deſerve it. And as it has been a faſhion ſince the publication of the famous ſtring of queries at the end of Sir Iſaac Newton's Optics, for ſpeculators to uſe that modeſt term to dignify inductions which they may ſuppoſe to be almoſt proved, I will take the liberty to offer a few on the preſent occaſion.

Apologetical remark.

1. Since the ſpecific gravity of ſteel ſuddenly cooled, is leſs than when annealed or ſlowly reduced in temperature; ſince ice and other cryſtals ſlowly formed, are generally underſtood to be denſer than the products of haſty refrigeration; and ſince in our experiment wax obeys the ſame law, is it not probable that the law may be general in the cooling of all bodies. *Fiant Experimenta.*

Qu. 1. Are not all bodies rarer if more ſpeedily cooled, congealed or cryſtallized?

2. Can this effect be aſcribed to any thing but the arrangement of the parts of bodies; and if ſo, is it not referable to haſty and ſlow cryſtallization; and are we not therefore juſtifiable in ſuppoſing that the cryſtallization of bodies may be altered, even in the ſolid ſtate, as in the hardening or ſoftening of ſteel.

Qu. 2. Is not this cryſtallization? even in bodies already ſolid?

3. As the hardneſs of ſteel becomes greater and its tenacity leſs by ſudden refrigeration, is it not probable that all the products of haſty cryſtallization are harder as they are known to be more brittle. Try in ſal ammoniac whether its ſoftneſs or flexibility will become leſs by ſudden cooling, ſuch as ſubli-

Qu. 3. Are not all theſe bodies more hard and leſs tenacious?

mation in an head covered with ice; and examine whether an unannealed glafs tube be not harder than a portion of the fame tube flowly cooled.

Qu. 4. Will it not be ufeful to repeat this experiment with other bodies?

4. May not fome ufeful indications as to the properties of metals and other bodies be derived from the fimple procefs of cafting a long flip of them, by pouring the fufed metal upon a cold ftone. If the curvature be univerfally like that of the wax, the law will be more and more confirmed. If anomalies prefent themfelves, our acquaintance with natural events will neverthelefs be extended and improved.

Experiments.

Since writing the above, I have taken fome of the materials next at hand to make an experiment or two, which I give without regarding whether they fupport, modify, or deftroy the hypothefis advanced above.

Exp. 1. Annealed and unannealed glafs, The annealed feems hardeft.

Exp. 1. A thick glafs tube which had been bended into a fyphon, was broken in two at the place of flexure. It was fuppofed that the bended part having been heated and cooled a fecond time, might prove fofter from this kind of annealing. The extremity of the ftrait part was applied to fcratch the bended part, and alfo the other ftrait part; and contrarywife the extremity of one of the bended parts was applied to the other portion of tube; and laftly both ends were fcratched with a file. No certainty was obtained, but it was thought that the bended part was hardeft *.

Exp. 2. Lead caft on a flab. No refult.

Exp. 2. Lead was fufed and poured red hot upon a marble flab in a long flip. Other lead moderately heated was poured out. The pieces did not quit the face of the ftone in cooling, and they were too flexible to be taken up and examined.

Exp. 3. Type metal. Bended like the wax.

Exp. 3. Type metal was treated in the fame manner. A flat piece one thirtieth of an inch thick, and fix inches long, bended upwards from the ftone in cooling, to the height or verfed fine of one twentieth of an inch, and retained its flexure.

Exp. 4. Fufible metal. Bended very much when nearly cold, *contrary to the wax, and recovered itfelf afterwards.*

Exp. 4. Fufible metal or the compound of lead, tin, and bifmuth, was poured out in a flip 13 inches long, one half being about the fame thicknefs or lefs than the type metal,

* As the tube had been bended with the blow pipe, the difference might as well have arifen from oxigenation of the lead or manganefe, or diffipation of alkali, as from fimple heating and cooling.
R. B.

and

and the other half confiderably thicker. It did not appear to rife from the flab; but was taken up while quite warm and held with its edge downwards, to prevent any flexure from its weight. The thin part immediately began to bend, and the thick part foon afterwards, *the flexures being concave on the fide which had touched the ftone,* and the verfed fine or height of the curvature was one inch at the end of half a minute, after which it gradually ftraightened itfelf as follows,

Bar of Fufible Metal 13 *Inches long.*

At $8^h\ 51^m$	taken up ftrait.		inch.
$51\frac{1}{2}$	it became bended;	verfed fine	1.00
54	- - lefs bended	- do. -	0.7
55	- - ————	————	0.4
57	- - ————	————	0.25
60	thin part ftrait; thick a little curved.		

Another experiment had been made before, without any expectation of fuch or fo great a flexure, and no meafures were then taken. In this the bar was thinner and the flexure greater; and the return to ftraitnefs was not only complete, but one end which was very thin became at laft bended the contrary way, namely the face that had touched the ftone was convex.

A fmaller piece of two inches in length which was not taken up fo foon, had become bended as it lay; fo that it refted on its two extremities and was hollow in the middle. This recovered its firft ftraitnefs as it lay on the table.

I am

S I R,

your obliged reader,

R. B.

IX.

An Account of some Cases of the Production of Colours, not hitherto described. By THOMAS YOUNG, *M. D. F. R. S. F. L. S. Professor of Natural Philosophy, in the Royal Institution* *.

Simple and general law respecting two portions of the same light arriving by different routes;

WHATEVER opinion may be entertained of the theory of light and colours which I have lately had the honour of submitting to the Royal Society, it must at any rate be allowed that it has given birth to the discovery of a simple and general law, capable of explaining a number of the phenomena of coloured light, which, without this law, would remain insulated and unintelligible. The law is, that "wherever two portions of "the same light arrive at the eye by different routes, either "exactly or very nearly in the same direction, the light becomes most intense when the difference of the routes is any "multiple of a certain length, and least intense in the intermediate state of the interfering portions; and this length is "different for light of different colours."

which explains many phenomena.

I have already shown in detail, the sufficiency of this law for explaining all the phenomena described in the second and third books of Newton's Optics, as well as some others not mentioned by Newton. But it is still more satisfactory to observe its conformity to other facts, which constitute new and distinct classes of phenomena, and which could scarcely have agreed so well with any anterior law, if that law had been erroneous or imaginary: these are, the colours of fibres, and the colours of mixed plates.

and new facts.

Production of colours by a minute fibre near the edge of an obstacle intercepting light &c.

As I was observing the appearance of the fine parallel lines of light which are seen upon the margin of an object held near the eye, so as to intercept the greater part of the light of a distant luminous object, and which are produced by the fringes caused by the inflection of light already known, I observed that they were sometimes accompanied by coloured fringes, much broader and more distinct; and I soon found, that these broader fringes were occasioned by the accidental interposition of a hair. In order to make them more distinct, I employed a horse-hair; but they were then no longer visible. With a

* Philos. Trans. 1802.

fibre of wool, on the contrary, they became very large and conſpicuous: and, with a ſingle ſilk-worm's thread, their magnitude was ſo much increaſed, that two or three of them ſeemed to occupy the whole field of view. They appeared to extend on each ſide of the candle, in the ſame order as the colours of thin plates, ſeen by tranſmitted light. It occurred to me, that their cauſe muſt be ſought in the interference of two portions of light, one reflected upon the fibre, the other bending round its oppoſite ſide, and at laſt coinciding nearly in direction with the former portion; that accordingly as both portions deviated more from a rectilinear direction, the difference of the length of their paths would become gradually greater and greater, and would conſequently produce the appearances of colour uſual in ſuch caſes; that, ſuppoſing them to be inflected at right angles, the difference would amount nearly to the diameter of the fibre, and that this difference muſt conſequently be ſmaller as the fibre became ſmaller; and, the number of fringes in a right angle becoming ſmaller, that their angular diſtances would conſequently become greater, and the whole appearance would be dilated. It was eaſy to calculate, that for the light leaſt inflected the difference of the paths would be to the diameter of the fibre, very nearly as the deviation of the ray, at any point, from the rectilinear direction, to its diſtance from the fibre.

Remarks and inferences. A portion of light reflected from one ſide nearly coincides in direction with another portion inflected; but their lengths differ and produce colour.

I therefore made a rectangular hole in a card, and bent its ends ſo as to ſupport a hair parallel to the ſides of the hole; then, upon applying the eye near the hole, the hair of courſe appeared dilated by indiſtinct viſion into a ſurface, of which the breadth was determined by the diſtance of the hair and the magnitude of the hole, independently of the temporary aperture of the pupil. When the hair approached ſo near to the direction of the margin of a candle that the inflected light was ſufficiently copious to produce a ſenſible effect, the fringes began to appear; and it was eaſy to eſtimate the proportion of their breadth to the apparent breadth of the hair, acroſs the image of which they extended. I found that ſix of the brighteſt red fringes, nearly at equal diſtances, occupied the whole of that image. The breadth of the aperture was $\frac{66}{1000}$, and its diſtance from the hair $\frac{8}{10}$ of an inch: the diameter of the hair was leſs than $\frac{1}{500}$ of an inch; as nearly as I could aſcertain, it was $\frac{1}{600}$. Hence we have $\frac{11}{1000}$ for the

Preciſe repetition of the experiment

the deviation of the firft red fringe at the diftance $\frac{8}{10}$; and, as $\frac{8}{10} : \frac{11}{1000} :: \frac{1}{600} : \frac{11}{480000}$, or $\frac{1}{43636}$ for the difference of the routes of the red light where it was moft intenfe. The meafure deduced from Newton's experiments is $\frac{1}{39200}$. I thought this coincidence, with only an error of one-ninth of fo minute a quantity, fufficiently perfect to warrant completely the explanation of the phenomenon, and even to render a repetition of the experiment unneceffary; for there are feveral circumftances which make it difficult to calculate much more precifely what ought to be the refult of the meafurement.

The halos round a diftant candle feen through wool.

When a number of fibres of the fame kind, for inftance, a uniform lock of wool, are held near to the eye, we fee an appearance of halos furrounding a diftant candle: but their brilliancy, and even their exiftence, depends on the uniformity of the dimenfions of the fibres; and they are larger as the fibres are fmaller. It is obvious that they are the immediate confequences of the coincidence of a number of fringes of the fame fize, which, as the fibres are arranged in all imaginable directions muft neceffarily furround the luminous object at equal diftances on all fides, and conftitute circular fringes.

Coloured atmofpheric halos.

There can be little doubt that the coloured atmofpherical halos are of the fame kind: their appearance muft depend on the exiftence of a number of particles of water, of equal dimenfions, and in a proper pofition, with refpect to the luminary and to the eye. As there is no natural limit to the magnitude of the fpherules of water, we may expect thefe halos to vary without limit in their diameters; and, accordingly, Mr. Jordan has obferved that their dimenfions are exceedingly various, and has remarked that they frequently change during the time of obfervation.

New colours feen through two plates of glafs with a little moifture between them mixed with air.

I firft noticed the colours of mixed plates, in looking at a candle through two pieces of plate-glafs, with a little moifture between them. I obferved an appearance of fringes refembling the common colours of thin plates; and, upon looking for the fringes by reflection, I found that thefe new fringes were always in the fame direction as the other fringes, but many times larger. By examining the glaffes with a magnifier, I perceived that wherever thefe fringes were vifible, the moifture was intermixed with portions of air, producing an appearance fimilar to dew. I then fuppofed that the origin of the colours was the fame as that of the colours of halos; but, on

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a more minute examination, I found that the magnitude of the portions of air and water was by no means uniform, and that the explanation was therefore inadmiffible. It was, however, eafy to find two portions of light fufficient for the production of thefe fringes; for, the light tranfmitted through the water, moving in it with a velocity different from that of the light paffing through the interftices filled only with air, the two portions would interfere with each other, and produce effects of colour according to the general law. The ratio of the velocities in water and in air, is that of 3 to 4; the fringes ought therefore to appear where the thicknefs is 6 times as great as that which correfponds to the fame colour in the common cafe of thin plates; and, upon making the experiment with a plane glafs and a lens flightly convex, I found the fixth dark circle actually of the fame diameter as the firft in the new fringes. The colours are alfo very eafily produced, when butter or tallow is fubftituted for water; and the rings then become fmaller, on account of the greater refractive denfity of the oils: but, when water is added, fo as to fill up the interftices of the oil, the rings are very much enlarged; for here the difference only of the velocities in water and in oil is to be confidered, and this is much fmaller than the difference between air and water. All thefe circumftances are fufficient to fatisfy us with refpect to the truth of the explanation; and it is ftill more confirmed by the effect of inclining the plates to the direction of the light; for then, inftead of dilating, like the colours of thin plates, thefe rings contract: and this is the obvious confequence of an increafe of the length of the paths of the light, which now traverfes both mediums obliquely; and the effect is every where the fame as that of a thicker plate.

Thefe were not produced by inflection and reflection but by the different velocities of the light through water and air.

Proofs of this theory.

It muft however be obferved, that the colours are not produced in the whole light that is tranfmitted through the mediums: a fmall portion only of each pencil, paffing through the water contiguous to the edges of the particle, is fufficiently coincident with the light tranfmitted by the neighbouring portions of air, to produce the neceffary interference; and it is eafy to fhow that, on account of the natural concavity of the furface of each portion of the fluid adhering to the two pieces of glafs, a confiderable portion of the light which is beginning to pafs through the water will be diffipated laterally by reflection

The curved figure of the particles caufe much of the light to deviate and modify the effects.

tion at its entrance, and that much of the light paffing through the air will be fcattered by refraction at the fecond furface. For thefe reafons, the fringes are feen when the plates are not directly interpofed between the eye and the luminous object; and, on account of the abfence of foreign light, even more diftinctly than when they are in the fame right line with that object. And, if we remove the plates to a confiderable diftance out of this line, the rings are ftill vifible, and become larger than before; for here the actual route of the light paffing through the air, is longer than that of the light paffing more obliquely through the water, and the difference in the times of paffage is leffened. It is however impoffible to be quite confident with refpect to the caufes of thefe minute variations, without fome means of afcertaining accurately the forms of the diffipating furfaces.

The arguments from the general law fhew that the velocity of undulation is greateft in rare mediums.

In applying the general law of interference to thefe colours, as well as to thofe of thin plates already known, I muft confefs that it is impoffible to avoid another fuppofition, which is a part of the undulatory theory, that is, that the velocity of light is the greater, the rarer the medium; and that there is alfo a condition annexed to the explanation of the colours of thin plates, which involves another part of the fame theory, that is, that where one of the portions of light has been reflected at the furface of a rarer medium, it muft be fuppofed to be retarded one half of the appropriate interval, for inftance, in the central black fpot of a foap-bubble, where the actual lengths of the paths very nearly coincide, but the effect is the fame as if one of the portions had been fo retarded as to deftroy the other.

The central black fpot on a foap bubble is produced by undulations reflected from the confines of a denfe and a rare medium in circumftances to deftroy each other:

From confidering the nature of this circumftance, I ventured to predict, that if the two reflections were of the fame kind, made at the furfaces of a thin plate, of a denfity intermediate between the denfities of the mediums containing it, the effect would be reverfed, and the central fpot, inftead of black, would become white; and I have now the pleafure of ftating, that I have fully verified this prediction, by interpofing a drop of oil of faffafrafs between a prifm of flint-glafs and a lens of crown glafs: the central fpot feen by reflected light was white, and furrounded by a dark ring. It was however neceffary to ufe fome force, in order to produce a contact fufficiently intimate; and the white fpot differed, even at laft, in the fame degree from perfect whitenefs, as the black fpot ufually does from perfect blacknefs.

a contrary effect is produced when both reflections are from a rarer medium.

The

The colours of mixed plates ſuggeſt to me an idea which appears to lead to an explanation of the diſperſion of colours by refraction, more ſimple and ſatisfactory than that which I advanced in the laſt Bakerian lecture. We may ſuppoſe that every refractive medium tranſmits the undulations conſtituting light in two ſeparate portions, one paſſing through its ultimate particles, and the other through its pores; and that theſe portions re-unite continually, after each ſucceſſive ſeparation, the one having preceded the other by a very minute but conſtant interval, depending on the regular arrangement of the particles of a homogenous medium. Now, if theſe two portions were always equal, each point of the undulations reſulting from their re-union, would always be found half way between the places of the correſponding point in the ſeparate portions; but, ſuppoſing the preceding portion to be the ſmaller, the newly combined undulation will be leſs advanced than if both had been equal, and the difference of its place will depend, not only on the difference of the length of the two routes, which will be conſtant for all the undulations, but alſo on the law and magnitude of thoſe undulations; ſo that the larger undulations will be ſomewhat further advanced after each re-union than the ſmaller ones, and, the ſame operation recurring at every particle of the medium, the whole progreſs of the larger undulations will be more rapid than that of the ſmaller; hence the deviation, in conſequence of the retardation of the motion of light in a denſer medium, will of courſe be greater for the ſmaller than for the larger undulations. Aſſuming the law of the harmonic curve for the motions of the particles, we might without much difficulty reduce this conjecture to a compariſon with experiment; but it would be neceſſary, in order to warrant our concluſions, to be provided with very accurate meaſures of the refractive and diſperſive powers of various ſubſtances, for rays of all deſcriptions.

Diſperſion of colours by refraction explained from the aſſumptions that light paſſes through the particles and through the pores.

Dr. Wollaſton's very intereſting obſervations would furniſh great aſſiſtance in this inquiry, when compared with the ſeparation of colours by thin plates. I have repeated his experiments on the ſpectrum with perfect ſucceſs, and have made ſome attempts to procure comparative meaſures from thin plates; and I have found that, as Sir Iſaac Newton has already obſerved, the blue and violet light is more diſperſed by refraction, than in proportion to the difference of the appropriate dimenſions

Compariſon of Dr. Wollaſton's obſ. with the ſeparation of colours by thin plates.

dimensions deduced from the phenomena of thin plates. Hence it happens, that when a line of the light proceeding to form an image of the rings of colours of thin plates, is intercepted by a prism, and an actual picture is formed, resembling the scale delineated by Newton from theory, for estimating the colours of particles of given dimensions, the oblique spectrums, formed by the different colours of each series, are not straight, but curved, the lateral refraction of the prism separating the violet end more widely than the red. The thickness corresponding to the extreme red, the line of yellow, bright green, bright blue, and extreme violet, I found to be inversely as the numbers 27, 30, 35, 40, and 45, respectively. In consequence of Dr. Wollaston's correction of the description of the prismatic spectrum, compared with these observations, it becomes necessary to modify the supposition that I advanced in the last Bakerian lecture, respecting the proportions of the sympathetic fibres of the retina; substituting red, green, and violet, for red, yellow, and blue, and the numbers 7, 6, and 5, for 8, 7, and 6.

Subdivision of the light of a candle explained.

The same prismatic analysis of the colours of thin plates, appears to furnish a satisfactory explanation of the subdivision of the light of the lower part of a candle: for, in fact, the light transmitted through every part of a thin plate, is divided in a similar manner into distinct portions, increasing in number with the thickness of the plate, until they become too minute to be visible. At the thickness corresponding to the ninth or tenth portion of red light, the number of portions of different colours is five; and their proportions, as exhibited by refraction, are nearly the same as in the light of a candle, the violet being the broadest. We have only to suppose each particle of tallow to be, at its first evaporation, of such dimensions as to produce the same effect as the thin plate of air at this point, where it is about $\frac{1}{10000}$ of an inch in thickness, and to reflect, or perhaps rather to transmit, the mixed light produced by the incipient combustion around it, and we shall have a light completely resembling that which Dr. Wollaston has observed. There appears to be also a fine line of strong yellow light, separate from the general spectrum, principally derived from the most superficial combustion at the margin of the flame, and increasing in quantity as the flame ascends. Similar circumstances might undoubtedly be found in other cases of the production or modification of light; and experiments upon this subject might

might tend greatly to eftablifh the Newtonian opinion, that the colours of all natural bodies are fimilar in their origin to thofe of thin plates; an opinion which appears to do the higheft honour to the fagacity of its author, and indeed to form a very confiderable ftep in our advances towards an acquaintance with the intimate conftitution and arrangement of material fubftances.

Difperfive powers of the eye.

I have lately had an opportunity of confirming my former obfervations on the difperfive powers of the eye. I find that at the refpective diftances of 10 and 15 inches, the extreme red and extreme violet rays are fimilarly refracted, the difference being expreffed by a focal length of 30 inches. Now the interval between red and yellow is about one-fourth of the whole fpectrum; confequently, a focal length of 120 inches expreffes a power equivalent to the difperfion of the red and yellow, and this differs but little from 132, which was the refult of the obfervation already defcribed. I do not know that thefe experiments are more accurate than the former one; but I have repeated them feveral times under different circumftances, and I have no doubt but the difperfion of coloured light in the human eye is nearly fuch as I have ftated it. How it happens to be no greater, I cannot at prefent undertake to explain.

X.

A Memoir on the Wax Tree of Louifiana and Pennfylvania. By CHARLES LOUIS CADET, *of the College of Pharmacy at Paris* *.

Wax bearing plants.

A GREAT number of plants, fuch as the *croton febiferum*, the *tomex febifera* of *Loureiro*, the poplar, the alder, the pine, and feveral labiated plants, afford a concrete inflammable matter by decoction, more or lefs refembling tallow or wax, that is to fay, a fixed oil faturated with oxigen. The light matter, which is called the down of fruits, which filvers the furface of prunes and other ftone fruits, is wax, as Mr. Prouft has fhewn. But the tree which prefents this fubftance

* Tranflated from the Ann. de Chimie, XLIV. 140.

in

The most productive is the myrica cerifera. in the greatest abundance, and in more respects than one, is entitled to the attention of cultivators, is the *myrica cerifera*, or wax tree.

Its early history. We read in the History of the Academy of Sciences for the years 1722 and 1725, that M. Alexandre, surgeon, correspondent with M. Marian, had observed at Louisiana a tree of the size of a cherry tree, having the appearance of the myrtle, and bearing a grain of the size of coriander seed. These grains, of a grey ash colour, contain a small round hard kernel, which is covered with a shining wax that may be obtained by boiling the grains in water. This wax is drier and more friable than ours. The inhabitants make candles of it. M. Alexandre adds, "This grain is usually of a deep and beautiful lake colour, which by merely crushing with the fingers leaves them tinged, but this is only at a particular season."

The liquor in which the grain has been boiled, and from whence the wax is procured, having been poured out and evaporated to the consistence of an extract, M. Alexandre discovered that it checks the most obstinate dysenteries.

The advantageous properties that this tree appears to possess, ought to have induced philosophers to make enquiries to ascertain the various properties of the vegetable, and what attention its culture might require; it has been long considered merely as an object of curiosity.

Linnæus, in his vegetable system, only mentions the wax tree of Virginia, myrica cerifera, the leaves lanceolated as if dentated, and the stem arborescent.

Species of the myrica cerifera. On enquiring of Cit. Ventenat, whether there were several species, he informed me, that Ayton distinguishes two, viz.

1. The *myrica cerifera angustifolia*, which grows in Louisiana. This tree is very delicate, it flowers with difficulty in our green houses; and its grains are smaller than those of the following.

2. The *myrica cerifera latifolia*, which grows in Pensylvania, Carolina, and Virginia, does not rise so high as the former; it is perfectly naturalized in France. These two myrica are diæcous.

They are both cultivated in the *Muséum des Plantes*, and in the gardens of Citizens Cels and Lemonier (at Paris.)

Cit.

Cit. Michault admits of a third ſpecies of *myrica cerifera*, which he calls the dwarf wax tree. Cit. Ventenat believes that wax may be procured from all the myrica.

The authors who have mentioned theſe trees with ſome detail are Marſhal, tranſlated (into French) by Leſerme, Lepage-Duprat, and Toſcan, librarian at the Muſeum of Natural Hiſtory. A memoir inſerted by the laſt in his work intitled *L'Ami de la Nature*, ſhews the manner in which the vegetable wax is collected in the colonies. Authors who have written reſpecting it.

"Towards the end of autumn," ſays he, "when the berries are ripe, a man leaves his houſe, together with his family, to go to ſome iſland or bank on the ſea ſhore where the wax trees grows in abundance. He carries with him veſſels to boil the berries, and a hatchet to build a cottage where he may find ſhelter during his reſidence in this place, which is uſually three or four weeks. While he cuts down the trees his children gather the berries. A very fertile ſhrub will afford near ſeven pounds. When theſe are gathered the whole family employ themſelves in procuring the wax. They throw a certain quantity of berries into the kettle, and then pour a ſufficient quantity of water on them ſo as to cover them to a depth of about half a foot. They boil the whole, ſtirring the grains about and rubbing them againſt the ſides of the veſſel, in order that the wax may more eaſily come off. In a ſhort time it floats on the water like fat, and is collected with a ſpoon and ſtrained through a coarſe cloth to ſeparate it from any impurities which might be mixed with it. When no more wax can be obtained, they take the berries out with a ſkimmer, and put others into the ſame water; but it muſt be entirely changed the ſecond or third time, and in the mean time boiling water muſt be added as it evaporates, in order to avoid retarding the operation. When a conſiderable quantity of wax has been obtained by this means, it is laid upon a cloth to drain off the water with which it is ſtill mixed. It is then dried and melted a ſecond time, to render it more pure, and it is then formed into maſſes. Four pounds of berries afford about one of wax; that which is firſt obtained is generally yellow: but in the latter boilings it aſſumes a green colour from the pellicle with which the kernel of the berry is covered." Manner of collecting the wax in America.

The

It is there used for soap making.

The traveller Kalm, ſpeaking of the vegetable wax, ſays, that in the country where the wax tree grows they make excellent ſoap of it, which waſhes linen perfectly white.

The author obtains ſome of the wax;

Such are the notions which have been formed reſpecting the myrica; at leaſt no other obſervations had been publiſhed to my knowledge, when a naturaliſt favoured me with half a kilogramme (17½ oz. avoird.) of the vegetable wax of Louiſiana. I was deſirous of making a comparative analyſis of it with bees wax; but before I undertook this work, I wiſhed to ſee the tree and berry of the myrica. I ſaw this precious vegetable at the *Jardin des Plantes,* and I wrote to Cit. Deſhayes, a zealous botaniſt, who cultivates the myrica penſylvania at Rambouillet, to requeſt him to give me ſome information concerning it. He had the politeneſs to ſend me an anſwer with ſome of the berries, which I immediately examined.

and examines the grain.

This grain is a kind of berry of the ſize of a pepper corn; the outſide when it is ripe and freſh, is white, and covered with ſmall black aſperities which give it the appearance of ſhagreen. When it is rubbed in the hands, it renders them greaſy or unctuous.

Experiments to ſeparate the waxy part mechanically.

If one of theſe little berries be ſtrongly preſſed, it parts with a matter reſembling ſtarch, and mixed with ſmall brown round grains like fine gunpowder. The nut which remains bare has a very thick ligneous covering, and contains a dycotildonous kernel. By rubbing a handful of the berries on a ſieve of horſe hair, I obtained a grey powder, in which the eye diſtinguiſhes without the aſſiſtance of a magnifying glaſs, the ſmall brown grains I mentioned, mixed with a white powder.

Application of alcohol.

I put this powder into alcohol, which with the aſſiſtance of a ſlight heat, diſſolved all the white part, and left the black powder which I ſeparated. Water poured on this ſolution in alcohol formed a precipitate that floated on the ſurface of the liquid. I melted this and obtained a yellow wax reſembling that ſent me from Louiſiana. This experiment compleatly proves that the wax of the myrica is the white grumous matter that ſurrounds the berries.

Black powder from the ſurface of the berries.

The black powder which was ſeparated appeared to me to contain the colouring principle, and I hoped alſo to find in it

it the beautiful lake mentioned by M. Alexandre. With this notion I cruſhed the powder ſtrongly, and boiled it in a ſolution of acid ſulphate of alumine; but was very much ſurpriſed to obtain nothing but a liquor ſcarcely coloured, and by which the alumine, when precipitated by an alcali, was but ſlightly tinged.

I took another portion of this fine black powder, and infuſed it in alcohol, I ſoon obtained a tincture of the colour of wine lees, which on being heated, became as red as a ſtrong tincture of quinquina, or terra japonica. The reſult led me to think that the colouring principle was reſinous; but on adding water to it, I did not perceive that any precipitate was formed.

I then poured into this tincture water charged with ſulphate of alumina, and obtained ſlight precipitate. A ſolution of ſulphate of iron immediately formed ink.

The powder afforded an aſtringent ſolution by alcohol.

What may this aſtringent colouring principle be which is only ſoluble in alcohol, which is not precipitated by water, and has ſo little attraction for alumine? To diſcover it, it is neceſſary to make a courſe of experiments which the ſmall quantity of the ſubſtance I was in poſſeſſion of would not permit me to undertake. The aſtringent matter mentioned by M. Alexandre muſt be found in the decoction of the entire grain. To aſcertain this fact I boiled ſome in a ſilver ſaucepan, the decoction upon which floated a little of the wax, was of a greeniſh colour, its taſte ſlightly ſtyptic, and it precipitated black ferruginous ſolutions. I heated it in an iron veſſel for that purpoſe, and it immediately became black. To diſcover whether this property aroſe from the gallic acid alone, or from tanin, I mixed a little of the concentrated decoction with a ſolution of gelatine, but it afforded no precipitate.

It contained gallic acid but no tanin,

It is therefore to the conſiderable quantity of gallic acid which the berries of the myrica contain, that we muſt attribute its effect in dyſenteries. I therefore ſuppoſe that the bark and leaves of the tree muſt contain an extract much more aſtringent than the berries.

Examination of the wax.

The examination of the wax afforded extremely intereſting reſults.

In whatever manner the wax may be obtained; by the decoction of the grains, or the ſolution of the powder when precipitated from alcohol by water, this wax when melted is al-

It is greenifh yellow; firm, and more brittle than bees wax, burns well in candles, and emits a fragrant fmell.

ways of a greenifh yellow. It is of a firmer confiftence than bees wax, it is dry, and fufficiently friable to be pulverized. In a word, it is evidently more oxigenated than the wax prepared by bees. Candles made of the wax of the myrica afford a white flame, a good light, without fmoke, and do not gutter; they emit when quite frefh a balfamic odour, which the inhabitants of Louifiana confider as extremely wholefome for perfons in ill health. When diftilled in a retort this wax paffes over for the moft part in the form of butter. This portion is much whiter, and has no more confiftence than tallow. Another portion that was decompofed afforded a little water, with fome empyreumatic oil, and febacic acid. Much carbonated hidrogen gas, and carbonic acid gas were difengaged; there remained in the retort a black and coaly bitumen. Wax ufually is affected in the fame manner by diftillation.

In diftillation it is affected like bees wax.

Ether diffolves it better than alcohol.

I have already mentioned, that alcohol diffolves the wax of the myrica, but ether diffolves it better, and it feparates in the form of ftalagmites by the evaporation of the liquid; neither of them difcolour it. If this wax be boiled with weak fulphuric acid it becomes paler, but there is no evident combination of the acid with it. The yellow bees wax under the fame circumftances did not change colour.

Ox. mur. acid bleaches it.

Oxigenated muriatic acid renders both kinds of wax perfectly white. The vegetable wax is the moft difficult to be bleached.

It is foluble in ammonia;

Vegetable wax diffolves in ammonia: the folution is of a brown colour; a portion of the wax is faponified. Volatile alkali has much lefs action on bees wax.

and forms foap with fixed alkali.

Both kinds of wax when ftrongly agitated in a boiling folution of cauftic potafh, become white and form a real foap as Kalm obferved.

Its whitenefs in foap.

The whitenefs acquired by the wax during this faponification is not a new phenomenon. Chaptal in his procefs for bleaching by the vapor of alkaline leys, proves that the colouring principle of vegetables yields to the action of alkalis. Some chemifts have attributed this effect to the direct combination of the foda or potafh with the coloured extract, a combination which makes it almoft of a foapy quality, and renders it foluble.

Chemical obfervation on the oxigenation of the oil in foap.

I apprehend that in this operation, the alkali exercifes a double attraction on the oil or wax, firft directly with the conftituent

ftituent principles of the oil and then predifpofing; and favouring the combination of the atmofpheric oxigen with the oil or wax. I do not know whether my notion is original; but I deduced it from the obfervation of what paffes during the decompofition of a foap by an acid: the oil is always concrete and becomes oxigenated. It would be interefting for chemical theory to make, if it were poffible, a foap in a clofed apparatus the air of which might be examined after the experiment, or in different gafes not containing oxigen.

When the foap of myrica is decompofed a very white wax is obtained, but in a peculiar ftate rendering it unfit for our ufes. The veg. wax from its foap is in a peculiar ftate.

Litharge or the femi-vitreous oxide of lead diffolves very well in melted wax of Louifiana; it forms a very hard plaifter, but its confiftence may be diminifhed at pleafure by the addition of a little oil. If as there is reafon to fuppofe, the wax of myrica retains a portion of the aftringent principle afforded by the decoction of the berries, phyficians may perhaps difcover fome ufeful topical remedies in the compounds of this wax. Plaifters with veg. wax.

From what has been faid, we fee that the myrica may be of the greateft ufe to the arts. The wax which it affords is fufficiently abundant to recompenfe the care and expenfe of cultivating it. For a bufh in a full bearing yields from fix to feven pounds of kernels, one fourth of which may be obtained in wax. It is fuperior in quality to bees-wax. Great utility of this wax.

The aftringent principle of the myrica, extracted in the large way may be very ufeful in medicine and in the arts; it may to a certain extent be fubftituted inftead of nut galls in dyeing, hat making, and probably in certain proceffes of tanning. The colouring principle appears fufficiently folid to deferve fome attention; and if it be true that fome fine lakes have been obtained from it in Louifiana, why may we not expect advantages from it in painting. and of its aftringent matter.

Laftly when this wax fhall have become plentiful and cheap in the market it promifes great advantage in the fabrication of foap.

The art of bleaching this wax will alfo require a courfe of experimental refearch; if it be propofed to operate economically and in the large way. Two re-agents prefent themfelves to manufacturers; the fulphuric acid, and the oxigenated muriatic To bleach veg. wax.

muriatic acid. But as the wax does not ſink in theſe liquids, we muſt multiply the contacts either by ſlicing and ſprinkling it with the oxigenated muriatic acid, or by incloſing it in this divided ſtate in caſks into which the oxigenated muriatic gas ſhould be paſſed.

The preference given to the action of the ox. mur. of lime.

I ſhall propoſe a third which promiſes a ſpeedier effect. The wax in a very divided ſtate is to be ſtratified in a caſk, with ſuper-oxigenated muriate of lime; in this manner they are to be diſpoſed in ſtrata, and left for ſome time in contact without moiſture. The ſalt is afterwards to be decompoſed with water acidulated by the ſulphuric acid; taking care to pour the water a little at a time at different intervals, until there ſhall be no longer any perceptible diſengagement of muriatic gas; at which period a large quantity of water is to be added and the mixture agitated with a ſtaff. The inſoluble ſulphate of lime falls down by repoſe, while the bleached wax riſes and ſwims at the ſurface. This is to be waſhed and melted on the water bath.

On the culture of the tree in Europe.

I ſhall conclude this memoir by offering ſome notions reſpecting the culture of the *myrica penſylvanica*.

Cit. Deſhayes to whom I am indebted for the trials I have made has obſerved the wax tree for ſeveral years at Rambouillet. The following is what he writes to me on the ſubject.

It is perfectly at home in France.

" The *myrica (latifolia)* Ayton is here abſolutely in its native country; it is in the ſoil beſt ſuited to it, namely in a ſandy and blackiſh turf: we have ſixteen very flouriſhing wax trees. Their height is from four to five and ſix feet, and one of them a male is ſeven feet high. The berries are abundant almoſt every year, I ſay almoſt becauſe there are ſome years in which they have failed. In general this fruit thrives very well in the part of the Engliſh garden aſſigned to it.

and requires no care in its culture.

" The culture requires no care. Every year numerous ſhoots are taken off which riſe at the feet of the large wax trees. Theſe are planted in other parts of the garden at the diſtance of one metre or yard aſunder."

The grain may be ſowed in the ground in ſpring, and afterwards tranſplanted, but the proceſs would be too long. The myrica ſucceeds wherever the ſoil is light and rather moiſt. How many provinces are there in which this cultivation might become uſeful and give employment to lands at preſent nearly abandoned.

How great are the advantages our agriculture might expect from this acquisition, since the myrica has long ago been seen to flourish in the dry sands of Prussia. Cit. Thieboult of the academy of Berlin, has communicated this interesting fact to me in the following note. It even grows well in Prussia.

The late Mr. Sulzer author of the general dictionary of the fine arts, obtained from Frederic the Great a portion of waste ground of considerable extent on the banks of the Spree, half a league from Berlin, in a place called the Moabites. However ungrateful this soil might appear as it presented only a very scanty and thin turf, upon a fine and light sand, Mr. Sulzer converted it into a garden very agreeable and worthy of a philosopher. Among other remarkable things, he made a plantation of foreign trees composed of five long rows, in the direction from East to West. There were not two trees in succession of the same species; he had placed in the rows most exposed to the North, none but such as were loftiest and most capable of resisting the rigours of the climate, So that by proceeding from North to South, the first row presented only trees of about seventy feet in height, the second trees between twenty-five and thirty feet high, and so in succession in an amphitheatre, where all the trees enjoyed the sun, at least in a part, and the weakest were sheltered by those which were more hardy. Narrative of a plant in the garden of Mr. Sulzer.

It was in the southernmost row, that I observed a kind of bush only two or three feet high, which Mr. Sulzer called the wax tree. All the visitors took particular notice of this tree in preference to all others, on account of the delicious odour of its leaves, which they preserved a very long time.

Citizen Thieboult afterwards speaks of the extraction of the wax. This operation does not differ from that related by Mr. Alexander.

I have seen, adds he afterwards, a single candle of this wax perfume the three chambers which composed the particular apartment of Mr. Sulzer, not only during the time it was lighted, but also for the rest of the evening. Remarkable perfume from the candles of veg. wax.

Without doubt the myrica cultivated at Berlin, was more odoriferous than that which grows with us, for ours does not emit the same perfume. Mr. Sulzer had the project of making candles of this wax, not bleached, but covered with our wax for the sake of beauty. The heirs of the academician have

 sold

fold the garden, but the wax tree ftill remained. It was planted in 1770. Since the poffibility of naturalizing the *myrica cerifera* has been afcertained in the north, why fhould we neglect a vegetable of fuch value and importance which could not fail to profper in our fouthern departments, and demands much lefs care than our bee-hives. The fuccefsful experiments already made ought to excite the zeal of our cultivators.

Economical Remarks.

The government has already encouraged this branch of induftry, by ordering plantations to be made. There exifts at Orleans and at Rambouillet, two orchards of the wax tree which contain more than four hundred fhrubs. We cannot give too much publicity to fuch fatisfactory refults. Nothing is propagated with fo much flownefs as ufeful plants. A barren but picturefque tree, or an agreeable flower are foon adopted by the fafhion. They ornament the gardens of our modern Lucullufes, and the toilets of our Phrynes, while our indefatigable agriculturifts make vain efforts to enrich our gardens with a new gramineous plant, or to fill our barns with nourifhing cereal plants. The people has long rejected from prejudice both maize and the potatoe which have been fo highly ferviceable to the poor and to our foldiery. We no longer find in our forefts the food bearing oak, upon which our anceftors fubfifted. Let us hope that our cultivators will at laft open their eyes upon their true interefts, and that lefs enflaved by old practices they will not defpife the prefents which learned focieties are defirous of making for their profit, and the reputation and profperity of their country.

Outline

XI.

Outline of the Craniognomic Syſtem of Dr. Gall, Phyſician at Vienna. By Dr.* Bojames †.

THE deſire of finding in the external ſtructure of man, certain indications of his internal faculties, his paſſions, his moral diſpoſition, &c. has in the moſt remote as well as in modern times, engaged philoſophers to eſtabliſh ſyſtems of phyſiognomy, which have been more or leſs ſatisfactory. Various ſyſtems of phyſiognomy,

The moſt generally known are thoſe of Porta, Lavater, the theory of the facial angle, and laſtly the ſyſtem of Gall. of Porta, of Lavater, and of the facial angle.

With regard to the firſt, who has buſied himſelf in comparing the outlines of the figure of man, with that of brutes, obſervers have decided on its value, and conſider his principles as the product of a wild imagination; they have found them too haſty, very little eſtabliſhed on reaſonable obſervations, and abſolutely uncertain in application. Syſtem of Porta, haſty and inaccurate.

Lavater's ſyſtem has had more ſucceſs; but though we revere the genius of this man who was really a great obſerver, we cannot be ignorant of the looſe foundation on which all his opinions are built, and the mind is unſatisfied with truths which can only be appreciated by an imagination as exalted, and feelings as delicate as thoſe of the author. Lavater's ſyſtem founded on ſentiment, not ſcience.

The theory of the facial angle which comprehends a more ample field than the ſyſtem of Lavater, leaves us in uncertainty as to the detail of faculties, and gives us only general points of view. But it preſents this moſt important truth, that the facial angle increaſes in magnitude in equal proportion with the faculties of animals, and in this point it evidently agrees with the general reſults of Gall's ſyſtem. Theory of the facial angle true; but too general.

Without entering into a ſcrupulous detail of the laborious courſe which this learned philoſopher has followed, in order Syſtem of Gall its fundamental principles.

* This hiſtorical expoſition which does not in the leaſt tend to prove the truths of Gall's ſyſtem, ſhould not influence any one's judgment concerning it, as it will be confirmed by its author with ſolid reaſoning and convincing proofs.

It is likewiſe neceſſary to remark, that the ſentences marked with inverted commas do not reſt on the authority of Gall.

† Inſerted by him in the Encyclopedie Methodique, of the learned *Millin.*

to

to eftablifh certain bafes for a fcience hitherto fo hypothetical, I fhall content myfelf with a fhort examination of its fundamental principles, which are,

1ft. *The Brain is the material Organ of the Internal Faculties.*

The brain is the organ of the faculties.

Without endeavouring to decide upon the metaphyfical queftions refpecting the nature of the foul, or of that which may be fuppofed the occult caufe of the internal faculties, we are neverthelefs compelled to admit a material organ for their action.

Proofs.

Now when we remark that thefe faculties are found only where the brain exifts, that they are loft with it; that the diforders and injuries fuftained by this organ, very fenfibly influence their degree and their action; that the volume of the brain increafes in direct proportion with the faculties of animals, &c. when we obferve all this I fay, there is nothing of conjecture in fuppofing the brain to be their material and intermediate organ.

Note. It might here be objected that in many cafes individuals have loft a confiderable portion of the fubftance of the brain, without their faculties having been fenfibly diminifhed; but it muft be obferved, that in general the organs of the brain are double, and that the cafes are far from being accurately ftated or eftablifhed.

The brain confifts of independent organs.

2d. *The Brain contains different Organs independent* * *of each another, for the different Faculties.*

The internal faculties do not always exift in equal proportions with refpect to each other; there are men who have much intelligence without much memory, courage without circumfpection, and metaphyfical genius without being profound obfervers.

Proofs. The faculties are exerted independently of each other, &c.

Again the phenomena of dreams, of fomnambulifm, of madnefs, &c. prove that the internal faculties do not always act together, that fome are often extremely active, while others are at the fame time totally infenfible.

* This notion of independence does not deftroy the principle of animal organization, that all the parts have a mutual relation; it only indicates that the action of one organ does not abfolutely caufe the fame degree in another. B.

Thus

Thus in old age, and ſometimes in diſeaſes, madneſs for example, many faculties are loſt whilſt others ſubſiſt; and conſtant employment of the ſame faculty ſenſibly diminiſhes its energy; if we paſs to another we find it has all the force of which it is ſuſceptible, and when we again employ the firſt faculty, we find it has recovered its original vigour. Thus it is that when fatigued with an abſtract and philoſophical reading, we turn with pleaſure to poetry, and then afterwards apply again with equal attention to our former employ.

Theſe phenomena prove that the faculties are ſeparated and independent of one another, and we are induced to believe that the ſame is the caſe with their material organs.

Note. "We do not entirely adopt this notion of Gall, but on the contrary, we believe that the ſeparation of the material organs muſt be conſidered as the cauſe of the diſtinction of the internal faculties; at leaſt it ſeems to us, that by ſuppoſing them originally ſeparated, we cannot avoid the ſnare of the materialiſm which exiſts as ſoon as we ceaſe to conſider the ſpirit (l'eſprit) as unity."

Theological note.

3d. *The Developement of the Organs contained in the Cranium, is in direct Proportion with the Force of their correſponding Faculties.*

The organs of the brain are more developed the ſtronger the correſpondent faculties,

This principle dictated by analogy, depends on the axiom; that through all nature the faculties are found to be ever in proportion to their relative organs, and its truth is eminently proved by the particular obſervations of Gall.

It muſt however be obſerved, that exerciſe has a conſiderable influence on the force of the faculties, and that an organ but moderately developed, but which is often exerciſed, may afford a ſuperior faculty to that which accompanies an organ of great magnitude, but is never put in action. Thus we ſee men whoſe ſtructure is but moderately ſtrong, acquire by continual exerciſe powers ſuperior to others whoſe ſtructure is almoſt athletic.

and the more from exerciſe.

Note. I muſt here anticipate an opinion which ſeems to reſult immediately from this principle, but which is nevertheleſs falſe, that the volume of the brain is in direct proportion to the energy of its faculties. Obſervation has demonſtrated to Gall, that the power of the faculties can only be appreciated by the developement of the organs ſeparately, which form

A large brain does not indicate energy of faculties unleſs the organs be ſeparately developed.

form diſtinct eminences on the cranium, and that a cranium perfectly round, whatever may be its magnitude, never exhibits faculties either great or numerous.

Conjecture as to the reaſon.

I do not recollect that I ever heard Gall give his reaſons for the concluſion: "but I think we may conſider theſe heads in a ſtate ſimilar to that of obeſity, and as we do not judge of the muſcular ſtrength of a man or animal by the volume of its members, but by their particular developement, ſo I think we muſt judge of the force of the faculties by the developement of their reſpective organs."

The external figure of the cranium exhibits the organs and ſhews the power of the faculties, becauſe the form of the cranium depends on that of the brain.

Laſtly, the fourth principle which is the moſt important in the practice of the ſyſtem of Gall, is: *That we may judge of theſe different organs and their faculties by the exterior form of the cranium.*

The truth of this principle is founded on another; namely, that the formation of the cranium depends on that of the brain, a truth generally known and proved by the anteriority of the brain, and by the impreſſions or indentations within the cranium.

Remark.

Note. It is true that there are craniums in which there is an internal protuberance of the bone correſponding with the external projection; and this irregularity, which is ſometimes found to be a diſorder, moſt commonly in an advanced age, when the organs of the brain do not ſo powerfully reſiſt the cranium, throws a degree of uncertainty in the practice of the ſyſtem of Gall; but this is the fate of all our truths which are dictated by experience: theſe caſes, however, are not very frequent.

On theſe principles Gall has compared the organs of men and animals.

Guided by theſe principles Gall has examined nature; he has compared the craniums of men and animals, and thoſe of men of ſimilar and of different faculties. His reſearches have almoſt inconteſtibly proved, not only the truths above ſtated, but that the faculties of animals are ſimilar to thoſe of man; that what we call inſtinct in animals is alſo found in man; for example, love, cunning, circumſpection, courage, &c. that the quantity of organs is the ſtandard which fixes the generic difference of animals; their mutual proportion, that of the individuals; that the diſpoſition to any faculty which is originally given by nature, may be developed by exerciſe and favourable circumſtances, and ſometimes by diſorders; but that it can

never be created if not given by nature *; that the accumulation of organs is conſtantly made from back to front, and from below upwards, ſo that animals in proportion as they reſemble man in the quantity of their faculties, have the ſuperior and anterior part of the cranium more developed; and laſtly in man, the moſt perfect of animals, there are organs in the ſuperior and anterior parts of the frontal and parietal bones aſſigned to faculties which excluſively belong to him. "In this laſt point of view it is that the diſcoveries of Gall perfectly coincide with the theory of the facial angle, which appears to confirm their truth."

It is difficult to give an accurate and ſatisfactory account of the detail of this ſyſtem, and of the various organs which Dr. Gall has diſcovered, without ſtating the variety of facts and examples which he exhibits as evidence to prove what he advances; I will however undertake this enumeration, being perſuaded that it will in many reſpects ſhew the author's manner of reaſoning, and give a clear notion of the manner of proceeding required to obtain his reſults †. Detail of the ſyſtem.

1. *The Organ of Tenacity of Life.*

The firſt organ which the author thinks he has diſcovered is that of the tenacity of life, *tenacitas vitæ;* he ſuppoſes the *medulla oblongata* to be its ſeat; and as the circumference of the great aperture of the *occiput* is in direct proportion to the extent of the *medulla oblongata,* he judges of the intenſity of the life of an animal by the magnitude of this perforation. Organs. 1. Tenacity of life.

The obſervations which ſupport this opinion are, that the perforation is generally larger in the *cranium* of women than men; that it is conſtantly of greater extent in the cat, the otter, the beaver, the badger, &c. animals which are known to be extremely tenacious of life. And the moſt ſpeedy method of killing an animal is to divide the ſpinal marrow.

2. *Organ of Inſtinct for ſelf-preſervation.*

Forwarder than the *medulla oblongata* at the place where it quits the brain, the author ſuppoſes the *organ of love of life, or inſtinct for ſelf-preſervation* to exiſt. 2. Self-preſervation.

* It is neceſſary that the germ of any organ ſhould ſubſiſt in the embryo, in order to its ſubſequent developement as an organ.

† Compare the correſponding numbers with thoſe of Plate X.

As animals do not afford examples of ſuicide, he could only obtain proofs of this ſuppoſition in the human race, and ſeveral caſes of voluntary ſuicide in which this part of the brain was diſordered, have induced him to conſider it as the organ of this faculty; he does not however admit it as indiſputably eſtabliſhed, but waits for further examples to confirm the fact.

3. *Organ of the choice of Food.*

3. Choice of food. The author ſuppoſes the organs for the choice of food to be placed in the quadrijumal tubercles, the anterior of which are greater in carnivorous animals, the poſterior developed in the herbivoran, but are of equal ſize in the omnivorous animals.

4. *Cerebral Organs of the external Senſes.*

4. External ſenſes. The middle part of the baſe of the brain is appropriated to the external ſenſes. It is the region from which thoſe nerves iſſue which are diſtributed into the organs of thoſe ſenſes.

(To be continued.)

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Account of the Ventriloquiſm exhibited by M. Fitz-James.

Account of ventriloquiſm. THE reader will doubtleſs recollect ſome curious facts and obſervations on ventriloquiſm by Mr. Gough in our ſecond volume, page 125. That acute philoſopher, reaſoning from a few facts and ſtating the want of more, appears inclined to adopt the theory that the art of ventriloquiſm conſiſts in cauſing the voice to iſſue from the mouth only, and uttering it in ſuch a direction, that the hearer may receive the impreſſion of ſome echo with conſiderably more force than he receives the original ſound. He gives inſtances of this proceſs, particularly where the ſound of a ring of bells appears to change its direction accordingly, as the hearer by moving along, receives it from different reflecting ſurfaces, while the original ſound is interrupted by ſome obſtacle. Whether the echos in a room be at all likely to be ſo managed would admit of conſiderable doubt; and without having witneſſed any exhibition of this kind in the leaſt to be compared with the ſurpriſing narratives we occaſionally hear, I have always been ſtrongly diſpoſed

difpofed to think that the delufions with regard to the fuppofed direction of the voice in fuch cafes are not phyfical but moral; that is to fay, they have arifen from fomething in the nature of the fubject, or the pofition and action of the fpeaker, with the character, tone, and manner of fpeaking. On the prefent occafion I have the fatisfaction to give fome account of the performance of M. Fitz. James, one of the firft mafters of this art; who in addition to his very ftriking powers as a fpeaker and actor has the candour and liberality to explain the nature of his performance to his auditors. I was prefent a few evenings ago at a public exhibition, which continues to be repeated at *Dulau's* in Soho-fquare; and though my account of what I faw and heard cannot but be very imperfect, and far from exciting the furprize which the actual performance produces, it may neverthelefs be of utility to eftablifh a few principles, and remove fome errors refpecting this art. Account of ventriloquifm.

After a comic piece had been read by Monf. Volange, M. Fitz-James, who was fitting among the audience, went forward and expreffed his fufpicion, that the ventriloquifm was to be performed by the voices of perfons concealed under a platform which was covered with green cloth. Replies were given to his obfervations apparently from beneath that ftage; and he followed the voices with the action and manner of a perfon whofe curiofity was much excited, making remarks in his own voice, and anfwering rapidly and immediately in a voice which no one would have afcribed to him. He then addreffed a buft which appeared to anfwer his queftions in character, and after converfing with another buft in the fame manner, he turned round, and in a neat and perfpicuous fpeech explained the nature of the fubject of our attention: and from what he ftated and exhibited before us, it appeared that by long practice he had acquired the faculty of fpeaking during the infpiration of the breath with nearly the fame articulation, though not fo loud nor fo varioufly modulated as the ordinary voice formed by expiration of the air. The unufual voice being formed in the cavity of the lungs is very different in effect from the other. Perhaps it may iffue in a great meafure through the trunk of the individual. We fhould fcarcely be difpofed to afcribe any definite direction to it; and confequently are readily led to fuppofe it to come from the place beft adapted to what was faid. So that when he went to the door

Account of ventriloquifm.

door and * afked "Are you there," to a perfon fuppofed to be in the paffage, the anfwer in the unufual voice was immediately afcribed by the audience to a perfon actually in the paffage; and upon fhutting the door and withdrawing from it, when he turned round, directing his voice to the door and faid, "Stay there till I call you," the anfwer which was lower, and well adapted to the fuppofed diftance and obftacle interpofed, appeared ftill more ftrikingly to be out of the room. He then looked up to the cieling and called out in his own voice, "What are you doing above? do you intend to come down?" to which an immediate anfwer was given, which feemed to be in the room above, "I am coming down directly." The fame deception was practifed on the fuppofition of a perfon being under the floor, who anfwered in the unufual but a very different voice from the other, that he was down in the cellar putting away fome wine. An excellent deception of the watchman crying the hour in the ftreet, and approaching nearer the houfe till he came oppofite the window was practifed. Our attention was directed to the ftreet by the marked attention which Fitz-James himfelf appeared to pay to the found. He threw up the fafh and afked the hour, which was immediately anfwered in the fame tone, but clearer and louder; but upon his fhutting the window down again the watchman proceeded lefs audibly, and all at once the voice became very faint, and Fitz-James in his natural voice faid, "he has turned the corner." In all thefe inftances, as well as others, which were exhibited to the very great entertainment and furprize of the fpectators, the acute obferver will perceive that the direction of the found was imaginary, and arofe entirely from the well ftudied and fkilful combinations of the performer. Other fcenes which were to follow required the imagination to be too completely mifled to admit of the actor being feen. He went behind a folding fcreen in one corner of the room, when he counterfeited the knocking at a door. One perfon called from within, and was anfwered by a different perfon from without, who was admitted, and we found from the converfation of the parties, that the latter was in pain, and defirous of having a tooth extracted. The dialogue, and all the particulars of the operation that followed, would require a long difcourfe if I were to attempt to defcribe them to the reader. The imitation of the

* The whole performance was in French.

the natural and modulated voice of the operator encouraging, soothing, and talking with his patient; the confusion, terror, and apprehension of the sufferer; the inarticulate noises produced by the chairs and apparatus, upon the whole, constituted a mass of sound which produced a strange but comic effect. Loose observers would not have hesitated to assert, that they heard more than one voice at a time; and though this certainly could not be the case, and it did not appear so to me, yet the transitions were so instantaneous without the least pause between them, that the notion might very easily be generated. The removal of the screen satisfied the spectators that one performer had effected the whole. Account of ventriloquism.

The actor then proceeded to shew us specimens of his art as a mimic; and here the power he had acquired over the muscles of his face was fully as strange as the modulations of his voice. In several instances he caused the opposite muscles to act differently from each other; so that while one side of his face expressed mirth and laughter, the other side appeared to be weeping. About eight or ten faces were shewn to us in succession as he came from behind the screen, which together with the general habits and gait of the individual totally altered him. In one instance he was tall, thin, and melancholic; and the instant afterwards, with no greater interval of time than to pass round behind the screen, he appeared bloated with obesity, and staggering with fulness. The same man at one time exhibited his face simple, unaffected, and void of character, and the next moment it was covered with wrinkles expressing slyness, mirth, and whim of different descriptions. How far this discipline may be easy or difficult, I know not, but he certainly appeared to me to be far superior to the most practised masters of the countenance I have ever seen.

During this exhibition he imitated the sound of an organ; the ringing of a bell, the noises produced by the great hydraulic machine of Marli, and the opening and shutting of a snuff box.

His principal performance, however, consisted in the debates at the meeting of *Nauterre,* in which there were twenty different speakers, as is asserted in his advertisement; and certainly the number of different voices was very great. Much entertainment was afforded by the subject, which was taken

Account of ventriloquism.

taken from the late times of anarchy and convulsion in France; when the lowest, the most ignorant part of society, was called upon to decide the fate of a whole people by the energies of folly and brute violence. The fame remark may be applied to this debate, as to the other fcene refpecting tooth-drawing; namely, that the quick and fudden tranfitions, and the great differences in the voices gave the audience various notions, as well with regard to the number of fpeakers, as to their pofitions and the direction of their voices.

This account of a very celebrated ventriloquift may perhaps feem too minute and particular for a philofophical Journal, which is lefs devoted to do juftice to the talents of men, than to inveftigate the caufes of things. But where a ftriking delufion may lead to miftaken theories of found, I conceive it to be no fmall part of the argument, which is to fet the truth in a clear light, to fhew how numerous and extraordinary the acquifitions and the ability of the performer may be. There is likewife a point of delicacy arifing from the fufpicion of improper motives, when a public performer is fpoken of in terms of approval by a periodical writer. On this head, however, I feel fo little difficulty, that I am almoft in doubt whether I fhould obliterate the laft obfervation, or fuffer it to pafs.

Afcent of Mont Blanc and Mont Perdu.

Afcent of Mont Blanc by M. Forneret and Dortheren,

M. Forneret of Laufanne, and the Baron de Dortheren, have undertaken a new excurfion to the fummit of Mont Blanc. After two days travel, they arrived at the fummit, where the wind was fo turbulent that they were forced to fit together with their guides in a mafs to prevent their being precipitated. The cold was fix degrees below congelation, and with the rarity of the air affected their lungs in fo painful a manner, that they declare that no inducement fhould prevail on them to repeat this expedition. The enterprize was without any beneficial confequence to the fciences. *Bibliotheque de Sonini.*

by de Sauffure,

It was in the year 1787, that the celebrated De Sauffure, arrived at the fummit of Mont Blanc; and his voyage was confidered as a real conqueft for the natural fciences. Sonini has learned by a private letter, that the indefatigable Ramond, well known from his refearches in natural hiftory, has at laft reached the fummit of Mont Perdu, the giant of the Pyreneans,

of Mont Perdu by Ramond.

neans as Mont Blanc is that of the Alps. This mountain covered with glaciers and eternal ſnows, which is elevated more than eighteen hundred toiſes above the level of the ſea; and riſes higher than all the granitic Pics except Mont Blanc, muſt nevertheleſs be ranked among ternary mountains, ſince it contains the remains of ſea animals and quadrupeds. The efforts of Ramond to reach the ſummit have till now been conſtantly unſucceſsful, and he is the only one who has yet accompliſhed it. It preſents on all ſides threatening projections and ſteep precipices. This time, inſtead of directing his courſe from north to ſouth by aſcending the mountain on the French ſide, he travelled from ſouth to north by the ſlope which is directed towards Spain. He will ſoon publiſh the intereſting recital of his aſcenſion.

Experiment on Sound.

In the Journal laſt quoted, there is an account or notice of an experiment performed in the palace of the Tribunat, which as there is ſome obſcurity probably owing to the conciſeneſs of the narrative, I tranſlate literally.

Low Sounds ſaid to be conveyed to great diſtances.

Two figures of the ſize of life placed at the extremity of a very extenſive apartment, hear ſpeaking with a very low voice in whatever poſition and however diſtant the perſons who ſpeak to them may be, and they anſwer all queſtions in the moſt ſatisfactory manner.

This curious experiment proves to philoſophers, that it is poſſible to aſſiſt the organ of hearing in the ſame manner as inſtruments have been contrived to aſſiſt the ſight.

The author affirms, that it is poſſible to tranſmit intire diſcourſes uttered in a low voice to very remote diſtances, without any indication of the place of the ſpeaker.

Evaporation of Water at an elevated Temperature.

M. Leindenfroſt, in a diſſertation publiſhed in 1756, announced, that water loſes the quality of evaporating accordingly as the heat of the bodies upon which it falls, is augmented from the point of ebullition to the temperature of a white heat. A drop of water which he let fall into an iron ladle heated to whiteneſs, was firſt divided into ſeveral ſmall globules which afterwards united in one. Conſidering this with attention, he obſerved, that it turned with great ſwiftneſs

Experiments on evaporation.

Experiments on evaporation.

nefs on its axis, and became fmaller and fmaller. After 34 or 35 feconds it difappeared with noife. A fecond drop which fell into the fame ladle now fomewhat cooled, difappeared in nine or ten feconds; laftly, a third difappeared in three feconds. When the fpherule of water in the white hot ladle was touched with a cold body, it difappeared in an inftant.

Klaproth has repeated thefe experiments in the following manner. He took a highly polifhed iron ladle which he heated to whitenefs, and threw therein a drop of water, which became divided into feveral globules of different fizes, and foon united into a fingle fphere. As foon as this globule which exhibited the phenomena before defcribed, had difappeared, he let fall a fecond, after which a third, &c. and he obferved, that the duration of each was lefs the more the ladle was cooled. The following is the refult of two experiments. In the firft experiment the intenfity of the heat being greater than in the fecond, the degree at which the water moft fpeedily evaporates happened later.

1ft *Experiment.*	feconds.	2d *Experiment.*	feconds.
The firft drop lafted	40	The firft drop lafted - -	40
The fecond - - -	20	The fecond - - - -	14
The third - - -	6	The third - - - - - -	2
The fourth - - -	4	The fourth - - - - -	1
The fifth - - - -	2	The fifth - - - - - -	0
The fixth - - -	0		

Thefe experiments require to be made with much care. M. Klaproth obferves, that the flighteft circumftances produce variations in the duration of the drops.

Seven drops having been fucceffively thrown into the ladle heated to the neceffary degree, foon united into a globular mafs, which began its movement by a very rapid rotation. This mafs afterwards divided at top, when a fpot of white froth was feen at the upper part of the ball, and its edges appeared indented.

This experiment was made with ten drops, and afforded the fame refult; but when a greater number was employed, the mafs was unable to preferve its rotatory motion, and the whole of the water difappeared with a hiffing noife.

If inftead of the iron ladle or fpoon, a capfule of pure filver or of platina heated to whitenefs be ufed, the appearances are nearly the fame, but the duration of the balls is commonly longer.

Dr. Wollaston; on the oblique refraction of Iceland Crystal.

Fig. 1.

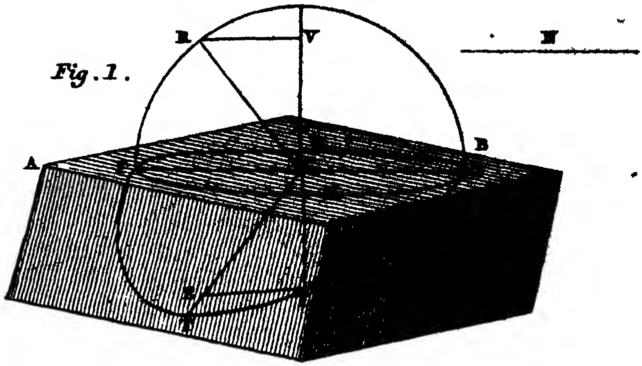


Fig. 2.

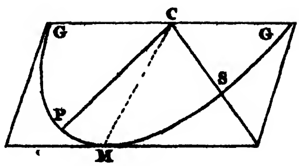


Fig. 3.

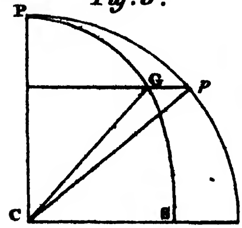


Fig. 4.

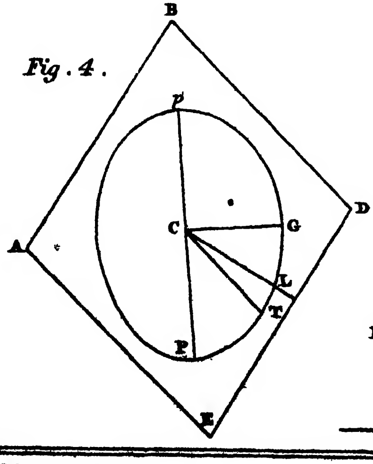
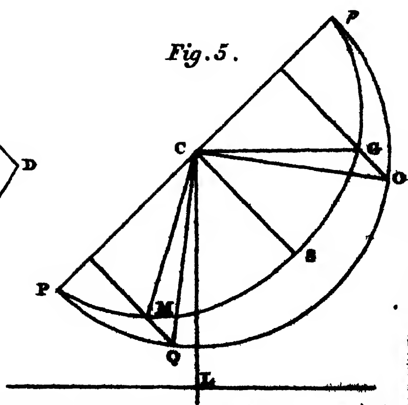
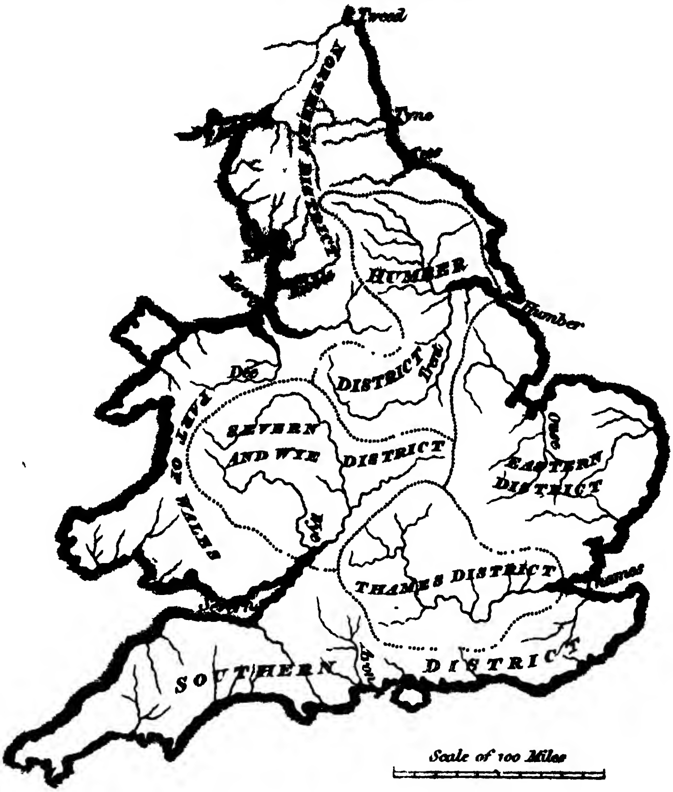


Fig. 5.



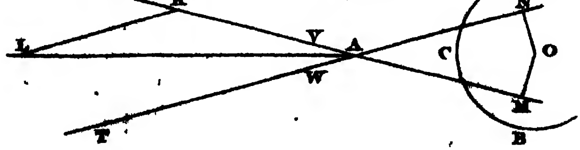
Mutlow Sc. Russell Co.

Mr. Dalton's Sketch of the Rivers in England & Wales; divided into districts.



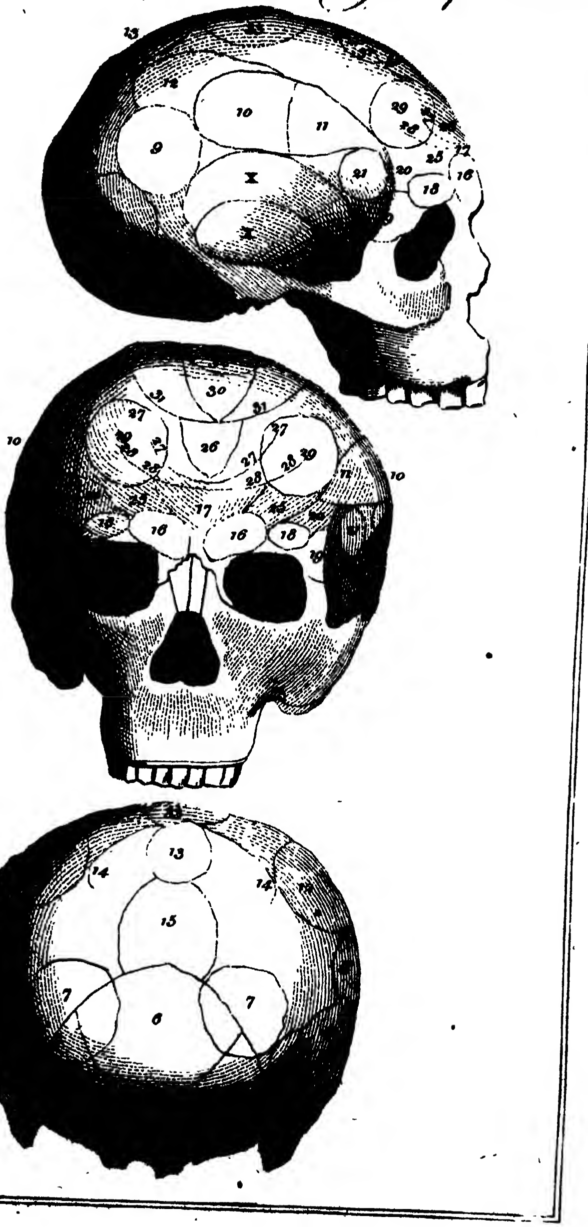
Mr. Gough, on sonorous Undulation.

Fig. 2.



Mutlow Sc. Russell Ct.

The craniognomic System of D.r Gall, of Vienna.



Mutlow Sc. Russell Co.t

Instrument for extracting hard substances lodged in the Throat.

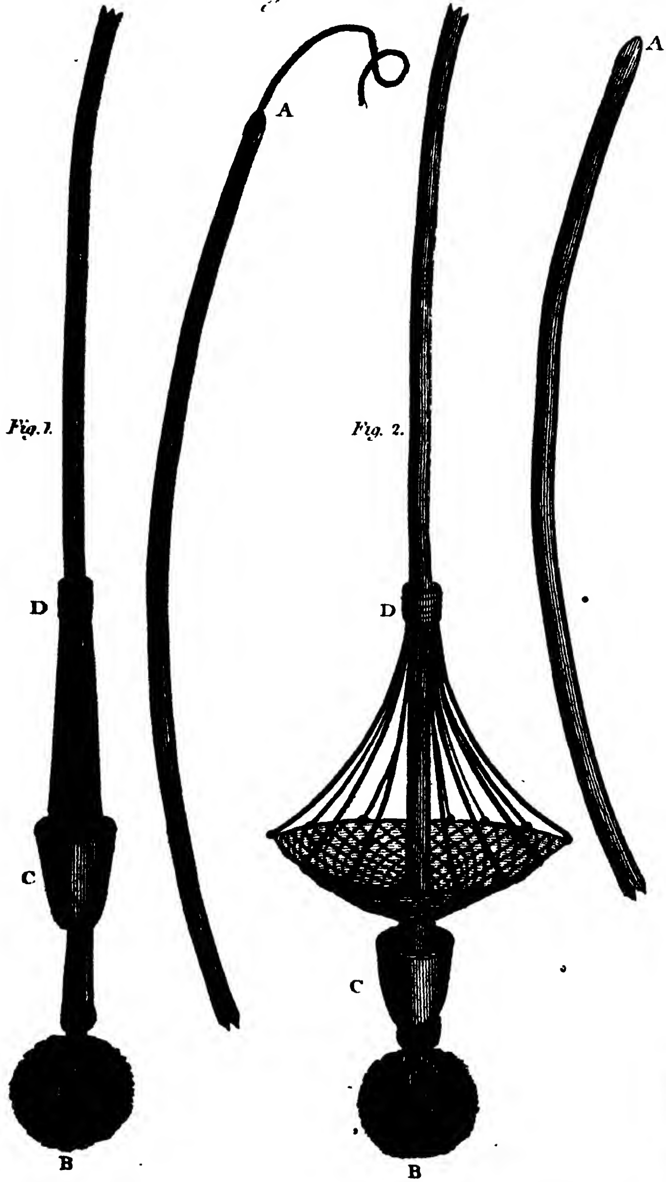


Fig. 1.

Fig. 2.

Medlow Sc. Russell Court.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

APRIL, 1803

ARTICLE I.

An Analysis of a Variety of the Corundum. By the Reverend WM. GREGOR. *Communicated by the Author.*

THE mineral, which is the subject of the following observations, was presented to me by my valuable and respected friend Philip Rashleigh, esq. who was struck with the peculiarity of its colour. Mr. Rashleigh informed me, that he had found its specific gravity to be 3,6 at the temperature 60, and my experiments gave the same result. History of th specimen of c rundum; fr Thibet.

The country where this stone was found, is Thibet: but in what particular part of that extensive region I am not informed.

"The colour of this stone is motled: the prevailing tint is a diluted lilac. The mass appears to be a confused crystallization, the parts of which are unequal, but mostly very minute. The larger grains reflect a lustre not inferior to that of the common adamantine spar. Although the adhesion of the parts of the mass is not considerable, the parts themselves have a great degree of induration, and from the same cause, probably, a greater specific gravity than the mass itself," which I found to be 3,603. Characters.

A.

Analyſis, pulverization, ignition, trituration.

100 grains of this ſtone, which had been reduced to a powder in a ſteel mortar, were not affected in the ſlighteſt degree by the magnet.

The powder was of a lilac inclining to a pink-colour. Expoſed to a ſtrong red heat for half an hour, it loſt $\frac{15}{16}$ of a grain in weight. Its colour was unaltered.—It was now rubbed (in a dry ſtate) to a finer powder in a mortar of flint. Upon weighing it again, I found that it had gained no acceſſion to its weight from the abraſion of it.

Boiled to dryneſs in potaſh; then waſhed; and the reſidue again treated and waſhed; and again, &c. till little remained.

It was now put into a ſilver-crucible, and covered with a ſolution of potaſh in * alkohol, mixed with an equal bulk of diſtilled water, and the crucible was placed in a ſand-bath, and the fluid gradually evaporated: and at laſt it was boiled to dryneſs. The ſalt which remained at the bottom of the crucible, was diſſolved in diſtilled water, and its ſolution poured off from a ſpungy earth, and a portion of the undecompoſed ſtone which had ſubſided. This operation was repeated in the ſame way with freſh portions of potaſh, until the whole of the ſtone was decompoſed, except a ſmall quantity of a ſpungy earth, which was thrown upon a filtre and waſhed with diſtilled water, until what paſſed through it ceaſed to cauſe the leaſt turbidneſs in a ſolution of nitrat of mercury. The edulcorating water was added to the ſolution which had been effected by potaſh.

B.

The undecompoſed reſidue was partly diſſolved by ſulphuric acid.

The powder which remained on the filtre was dried. It was of a greyiſh white colour. I put it into a ſmall matraſs and moiſtened it with diſtilled water, and then dropped ſome

* I have reaſon to ſubſcribe to the opinion of Mr. Chenevix, which he has given us in the maſterly and ſcientific paper on the arſeniats of copper, with which he has enriched the annals of mineralogy and chemical analyſis, that it is by the means of alkohol alone, that potaſh can be prepared, which is fit for delicate experiments. I was induced to employ a ſolution of potaſh in alkohol, becauſe it has been ſaid, that during the proceſs for obtaining it in a dry ſtate, it acts in ſome degree upon the ſilver veſſels, in which its ſolution is evaporated. By evaporating it *in contact with the ſpar*, I thought that this would be leſs likely to happen.

rectified

rectified ſulphuric acid upon it, and placed the veſſel in a digeſting heat. The powder became at firſt gelatinous. But this inconvenience was gradually removed, by the further addition of acid. What was diſſolved thereby was extracted by diſtilled water.

The laſt reſidue was again treated with potaſh, and its reſidue by ſulphuric acid. The ſolutions were added to the former, and a ſmall portion of ſilex remained.

The earth which remained, after having been ſufficiently edulcorated, was dried and expoſed to the action of potaſh in a ſilver crucible as before, and what the potaſh diſſolved was added to the alkaline ſolution before mentioned (A *b*). What was undiſſolved by it was heated again with ſulphuric acid, until it ceaſed to act upon it. The ſoluble part was extracted by diſtilled water. The reſiduum was thrown on a filtre and waſhed with diſtilled water till it ceaſed to affect nitrat of mercury: dried and heated red hot for half an hour, it weighed $\frac{1}{8}$ gr. It was pure ſilex.

The ſulph. ſolution was precip. by ammonia. It contained no lime.

The ſolution effected by ſulphuric acid, was mixed with the edulcorating water and decompoſed by ammonia. A white ſpongy earth was precipitated. When the ammonia ceaſed to cauſe any further precipitation, the clear decanted fluid was aſſayed with carbonat of ammonia and carbonat of potaſh, but no change was produced on it by the ſolution of either of thoſe ſalts. From which circumſtance the abſence of lime may be inferred.

The waſhed ammon. precip. again treated with potaſh; in part diſſolved.

The precipitate produced by ammonia was ſufficiently waſhed with diſtilled water, and in a moiſt ſtate, was boiled with the ſolution of potaſh in a ſilver crucible as before. The alkali diſſolved a portion of it. Which ſolution, together with the edulcorating water, which waſhed the remainder, was added to the alkaline ſolution before mentioned, (A *b*). What remained was thrown on a filtre, and edulcorated as before: dried and heated red hot for ten minutes, it weighed four grains.

It left oxide of titanium.

It was of the colour of pounded reſin. Expoſed to the flame of the blowpipe or charcoal it was unaltered. Moiſtened with melted tallow and ignited, it was not attracted by the magnet. It was taken up by a globule of the phoſphat of ammonia and ſoda, and ſuſpended in it in white flakes. It is ſoluble in the three mineral acids. But the ſolution, on being boiled, becomes turbid from the ſeparation of a large portion of it. It is precipitated from its ſolution in acids by

 pruſſiat

pruſſiat * of potaſh, of a beautiful graſs-green, and by tincture of galls of a dark orange-colour. It poſſeſſed, in ſhort, all the properties of the oxide of titanium.

C.

The alk. ſolutions ſat. with mur. acid, and diſſolved; precip. with carb. of potaſh; and edulcorated.

The alkaline ſolution (A *b*) was ſaturated with muriatic acid. A copious precipitation took place of a white earth, which was rediſſolved by a further addition of acid. The contents of this ſolution were thrown down by a ſolution of carbonat of potaſh. This precipitate collected on a filtre, was waſhed with diſtilled water, till it paſſed through it taſteleſs.

Dry precip. treated with ſulph. acid left ſilex.

It was dried and introduced into a matraſs, and rectified ſulphuric acid, diluted with diſtilled water, dropped into it, and the fluid was evaporated. Freſh portions of acid were abſtracted from it, as long as it ſeemed to act upon it. The ſoluble part was extracted by diſtilled water, and the reſiduum was edulcorated with diſtilled water, till it ceaſed to produce any change upon nitrat of mercury; dried, and heated red hot for half an hour, it weighed 10 grains. It was pure ſilex.

This ſulph. ſolution with acet. of potaſh gave alum.

The ſolution which the ſulphuric acid had effected, mixed with the water with which the reſiduum had been edulcorated, was gradually evaporated, and a ſufficient quantity of acetite of potaſh was added to it. Regular cryſtals of alum were formed to the laſt. A ſmall portion of ſilex, amounting to about $\frac{1}{2}$ a grain, was ſeparated.

The alum. was decomp. by carb. ammon.

Diſtilled water was poured upon the alum, in a quantity ſufficient to diſſolve it in a warm ſtate; and whilſt warm, the ſolution was decompoſed by a ſolution of carbonat of ammonia. The ſpungy earth which was ſeparated, was collected on a filtre and edulcorated with diſtilled water, till it produced no effect upon nitrat of mercury.

* The pruſſiat of potaſh which I employed, was prepared by dropping a ſolution of tartrite of potaſh into a ſolution of pruſſiat of lime, as long as any tartrite of lime is precipitated. Large and beautiful cryſtals of pruſſiat of potaſh are produced by evaporating the fluid, which I have found to remain unaltered for ſeveral years. By this method the intruſion of the ſulphuric acid is prevented: but Mr. Henry's late ingenious proceſs ſeems to be, for many reaſons, preferable to it. The pruſſiat of titanium promiſes I think, to be uſeful in the arts. Painters in oil and water colours would find in it a valuable acquiſition, as a beautiful tranſparent green.

The

The alumina was dried gradually, and heated in a crucible, ſo that the bottom of it was only of a low red: it weighed $111\frac{5}{8}$ gr. Expoſed to a ſtrong red heat for half an hour, it$=87\frac{3}{4}$. Suſpecting that a ſmall portion of alkali was retained by it, I rubbed it to a fine powder, and digeſted it with acetic acid, for many hours, and ſaturated it with ammonia. Precip. of earth freed from alk. by acet. acid and heat.

The alumina ſufficiently edulcorated, dried and expoſed to a ſtrong red heat for half an hour, now weighed $81\frac{3}{4}$. 100 grains of the original pounded ſpar, from which I had taken the 100 grains on which I operated, were expoſed to the ſame heat, and for the ſame time, and loſt $\frac{15}{16}$ of a grain, as I had found before.

The proportion which the ſeveral ingredients of this ſtone bear to each other, according to the foregoing analyſis, will be, Component parts.

Alumina - - - -	[C *d*]	- - - - - - -	81,75
	[B *b*]	1,625	
Silex - - - - - -	[C *b*]	10	12,125
	[C *c*]	0,50	
Oxide of titanium -	[B *d*]	- - - - - - -	4
Water - - - -	[A *a*]	- - - - - - -	0,937
Loſs - - - - - -		- - - - - - -	1,188
			100

I have repeated the analyſis of this mineral * ſeveral times, and have always found it to contain the ſame ingredients; though there has been a ſlight variation as to the relative quantities of each. This we might expect to be the caſe, not only from the loſs inſeparable from the operations to which the ſtone is ſubjected, but alſo from the nature of the ſtone itſelf, as it is not perfectly homogeneous. In one inſtance, I found the alumina amount to 84 per cent. and the ſilex proportionably diminiſhed.—Until we have better means than we have at preſent, of aſcertaining the real quantities of water and oxygen which the earths and the oxides of metals ſeverally retain in the Analyſis repeated.

* I am indebted to the kindneſs of Mr. Raſhleigh, for another ſpecimen of corundum from the ſame place, which differs in ſome reſpects in its external appearance from that which I examined. Its colour is not of ſo bright a lilac, and it ſeems of a more compact and uniform grain. Its ſurface is covered with a coat of yellow mica.—This I have not ſubjected to experiment.

the natural ſtate in which they exiſt, as ingredients of complex ſtones, we muſt conſider the reſult of chemical analyſis, but as an approximation towards the ſtrictneſs of truth.

As the intruſion of the oxide of titanium into the place, which has been uſually occupied by the oxide of iron, conſtitutes this ſtone a variety of the adamantine genus, I have been induced to record my experiments. I am aware, however, that it may be juſtly obſerved, that the oxide of neither metals is eſſential to the nature of the corundum.

Oxide of titanium often met with.

I have no doubt, but that the oxide of titanium will be found to be more plentifully ſcattered abroad throughout the mineral kingdom, than it has been hitherto imagined. I have detected it in a ſpecies of ſhirl, which occurs in a large cryſtallized maſs, on the tenement of Botallack, in the pariſh of Juſt, in this county. It compoſes ſeveral alternate beds or floors of varying dimenſions.—The compactneſs of theſe beds not having allowed the cryſtals to ſhoot with freedom in any direction; It is rarely that they occur with their terminations compleat.—Theſe, however, in no reſpect differ from the common form, and vary only in the length of the priſm, from $\frac{1}{4}$ of an inch to an inch.—The colour of this ſhirl is black.—I have alſo lately diſcovered the oxide of titanium in two ſpecies of baſalt, of the form of large pebbles, which I found near the ſea-ſhore. Whether they are natives, or whether they were dropped upon our coaſt accidentally as ballaſt from ſome veſſel, I have not as yet had an opportunity of aſcertaining. That they are *real baſalts*, I reſt upon the authority of one on whom I can ſafely rely, whoſe knowledge in mineralogy can be only equalled by his friendly diſpoſition to communicate it. I mean John Hawkins, Eſq.

My experiments alſo proved, that they contained all the known ingredients of baſalts, with the addition of the oxide of titanium.

Creed, near Grampound, Cornwall,
March 2, 1803.

Letter

II.

Letter from Mr. William Henry. *Concerning the Invention of Aromatic Vinegar.*

To Mr. NICHOLSON.

SIR,

I AM fenfible that an apology is neceffary for intruding on you and on your readers, a fubject which may appear, on firft view, to have little claim to general attention. If the rights and interefts of an individual were alone involved on this occafion, I fhould not have requefted a place in your Journal, for the following ftatement. But it is furely matter of general concern, that the appropriation of difcoveries and improvements fhould be dealt with ftrict juftice to their authors: for the profpect of this diftribution of " honour where it is due," is one of the moft animating principles of action; and the extinction of this motive would follow an indifference on the part of the public, to the claims of inventors.

The appropriation of difcoveries is matter of general concern.

More than fifteen years ago, during the delivery of a courfe of chemical lectures by my father, in this town, he had occafion to notice a quality of the acetic acid or radical vinegar, which had not to his knowledge been obferved before; viz. its property of diffolving camphor and various effential oils. The compound was found to poffefs a moft pungent and agreeable odour; and as the *vinaigre des quatres volcurs* had gained much reputation in preventing infection, it occurred to him that the newly difcovered folution would have ftill more powerful effects, in confequence of its high ftate of concentration. A bottle of this preparation he gave to a late active magiftrate and philanthropift (T. B. Bayley, efq. F. R. S.) who, in the courfe of an unwearied and undaunted exercife of his public functions, was frequently expofed to the dangers of foul and infected air. Mr. Bayley was highly gratified by its effects; and not only made conftant ufe of the aromatic vinegar on the bench, and on his vifits to the prifon, but introduced it to the adoption of feveral of the judges, and principal gentlemen at the bar. He alfo firft fuggefted to my father the propriety of benefiting by his difcovery, and was the medium of a connection with Mr. Bayley, perfumer, in Cockfpur Street, which has been continued to the prefent day.

Difcovery that acetic acid diffolves camphor and effential oils.

The

The sanction of an eminent physician given to a subsequent claimant.

The aromatic vinegar, like all other articles in general demand, has been a frequent subject of imitation. But it is not of this that I complain; for in consequence of unremitting attention, our preparation has maintained a decided superiority over all others, both as to quality and extent of sale. The occasion of this appeal to your readers is, that one of these imitations has been sanctioned by the name of a respectable physician, who, though not expressly yet by implication, confers on another the credit of that invention, which in justice is due to my father. *(See a letter from Dr. Trotter, physician to his majesty's fleet, contained in the advertisement of a London druggist.)*

Subsequent proceedings, &c.

From the recommendatory letter alluded to, it is sufficiently evident that Dr. Trotter was unacquainted with any prior claim to the invention of the aromatic vinegar; and he was therefore furnished by my father, in the most respectful manner, with the facts that have been laid before you. To this letter the doctor has never replied, though he declared verbally, to a medical gentleman, that my father's preparation had never fallen in his way; and that if it had, he should with equal readiness, have given testimony in its favour. The advertisement, however, still continues to be regularly inserted; and I therefore deem it expedient to appeal thus publicly, against the injustice of such a proceeding; especially in behalf of a man, who has imitated the original, only in assuming, with the coolest effrontery, an advertisement drawn up by myself.

I believe there are few of your readers, who will not decide, that the ordinary forms of civility required Dr. Trotter to have taken some notice of the letter that was addressed to him; that such an attention ought to have been paid to one of the oldest practitioners of medicine in this country; and that more respect was due to a man (whom I trust it is not unbecoming me to characterize, in terms already publicly applied to him, *viris laudatis* *) "respectable in science and literature," and "distinguished by ingenuity, honour, and the strictest integrity."

I am, Sir,

your obliged friend and servant,

WILLIAM HENRY.

Manchester, March 13, 1803.

* Dr. Aikin and Dr. Percival.

Caution

III

Caution against the Danger of leaving Phosphoric Preparations in the Vicinity of Wood. By a Correspondent.

To Mr. NICHOLSON.

March 1, 1803.

SIR,

ACCIDENTS in chemical experiments not feldom afford ufeful facts or fuggeftions; but I am doubtful whether or no the following occurrence will be thought worth notice in your Journal: however, the account of it is at your fervice.

Narrative of fire occafioned by oxidule of phofphorus.

While at lecture this morning, we were fuddenly annoyed by a column of white fumes iffuing from amongft bottles of preparations on a fhelf, which was foon followed by flame. On examination I found the inflammation proceeded from a bottle containing lime, into which phofphorus had fublimed in the upper part of a tube in the making of phofphuret of lime. This was not therefore phofphuret of lime, but only the mixture of particles of ignited phofphorus, with pulverized lime fcarcely ignited. In this circumftance the phofphorus I know becomes oxigenized to be in the ftate of *oxidule*, if not of *oxide;* which compofition it is well known, I fuppofe, is employed for the charging of little bottles to inflame fulphur matches by mere friction of them within fide of the bottle. In the prefent cafe, the bottle containing the lime with phofphorus had cracked, fo as to admit air, which excited the inflammation at the temperature of about 65°. Such preparations fhould therefore be kept out of the way of wood, or any inflammable fubftance, for if this accident had happened when no one was prefent, the whole laboratory would have been fet on fire.

Your's truly,

IV. *Defcription*

IV.

Description of a new Reflecting Quadrant. By Mr. Ezekiel Walker. *From the Author.*

To Mr. NICHOLSON.

SIR,

Obfervations on Hadley's quadrant.

THAT inftrument which goes by the name of Hadley's Quadrant, may perhaps be deemed one of the moft ufeful inventions of the laft century. Although its arc is only the one eighth part of a circle, yet it is fo conftructed as to meafure angles from 0° to 90° by the fore obfervation, and from 90° to 180° by the back obfervation. But as no method has yet been found out, by which the back horizon glafs can be adjufted with the fame exactnefs as the fore one, all angles above 90° which are taken by the back obfervation, cannot be relied on fo much as thofe that are taken by the fore obfervation. To obviate this inconvenience, the fextant was invented, by which any angle lefs than 120° may be taken by the fore obfervation. This is an invaluable inftrument to the nautical aftronomer; and indeed it is much to be regretted, that its ufe is not fo generally underftood by travellers, as the imperfect ftate of geography requires. It muft, however, be admitted, that it is not quite free from imperfections. Firft, an angle greater than 120° cannot be taken by it; and fecondly, when a large angle is taken, the rays of light fall fo obliquely upon the index glafs as may occafion fome doubt refpecting the truth of the obfervation. Mr. Ludlam gives it as a general rule, in conftructing an octant, "that very oblique reflections from the mirrors ought to be avoided *." If it be neceffary to obferve this rule in conftructing an octant, the fame fhould be attended to, as much as poffible, in taking obfervations.

Imperfections of the fextant.

New inftrument meafuring to 180° without oblique reflection.

Thefe imperfections fuggefted the idea of an inftrument to meafure any angle between 0° and 180° by the fore obfervation, without very oblique reflections from the mirrors. Plate XII. Fig. 2. exhibits a plan of this inftrument, where L M C reprefents an octant, conftructed in the ufual way, A B the

* Ludlam on the ufe of Hadley's quadrant, par. 91.

index

index glaſs, *m* the horizon glaſs, and H E the line of ſight, or axis of the teleſcope. Upon this line of ſight, at an angle of 45 degrees, let another horizon glaſs be fixed, repreſented by *x y*, which may be called *the ſecond horizon glaſs*.

When a ray of light R *n* falls upon the centre of the glaſs *x y*, at an angle of 45 degrees, it will be reflected to the eye at E; becauſe the angle *x n* E (the angle of reflection) is = 45° by conſtruction, and conſequently R *h* H and R *n* E are right angles.

The Uſe of the Reflecting Quadrant in taking Angles.

All angles leſs than 90° are to be taken by this inſtrument, in the ſame manner as by the ſextant. But, ſuppoſe it were required to obſerve the ſupplement of the ſun's meridian altitude at ſea, let the axis of viſion be directed to the zenith, and turn the quadrant till that part of the horizon, which is oppoſite to the ſun, be ſeen by reflection from the ſecond horizon glaſs; then turn the index from 0 towards M, till the ſun appears in the teleſcope, with one of its limbs in contact with the horizon, and the index will ſhew the ſun's obſerved zenith diſtance; to which add 90°, and the ſum will be = the angle obſerved by the quadrant, = the obſerved ſupplement of the ſun's meridian altitude. Manner of uſing it.

To adjuſt the Second Horizon Glaſs.

Let the arc L N be made equal to the arc L M, and graduated from 0° at L, to 90° at N. Then, after the index error has been determined, ſet the index to 90° at N, and elevate the teleſcope till the ſun, or ſome other remote object, be ſeen by reflection from the index and horizon glaſſes, and the ſame object will alſo be ſeen by reflection from the ſecond horizon glaſs: conſequently, the angle R *n* H becomes known to as great a degree of preciſion as the index error. Adjuſtment.

I am,

SIR,

Your obedient ſervant,

EZEKIEL WALKER.

Lynn Regis, Feb. 20, 1803.

V. *Miſcellaneous*

V.

Miscellaneous Information. Mistake respecting Dr. Thomson, Author of the Elements of Chemistry. Observations on the Scottish Querns. On the supposed Determination of the real Zero of Heat. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Mistake respecting Dr. Thomson.

I BEG leave to observe a small inaccuracy in the notice * of Dr. Thomson's System of Chemistry. Thomas Thomson, M. D. Lecturer on Chemistry in Edinburgh, who is much esteemed for his extensive knowledge, is the author of that excellent work. The Notes on Fourcroy's Elements of Chemistry were written by Mr. John Thomson, surgeon in Edinburgh.

The following remarks are at your service, from

SIR,

Your very humble servant,

H.

Edinburgh, Feb. 17. 1803.

1. *Observations on the Scottish Querns.*

The Scottish quern or hand mill.

The Indian hand mill, described in the Philosophical Journal (III. 186), very much resembles the hand mill or *querns* used by the cottagers in Scotland for grinding malt. There are many varieties of querns, but the greater number of them answer to the description just quoted, to which I beg leave to refer the reader.

The querns in Aberdeenshire have a peculiar form: a cylindrical cavity in the under stone contains the upper one. A plug of wood is driven in the center of this stone for fixing the gudgeon, and there is an eye or aperture in the angle through which the ground malt runs out. Instead of cutting so much out of the solid stone, it would be better to use a wooden drum. It is unnecessary for me to give a more minute detail of the Scottish querns, which have been long known in this

* Phil. Jour. 8vo. III. 62.

country;

country; feveral pieces of them having been found in the ruins of very old buildings.

In Dumfriesfhire there is a fmall pair of mill ftones on a frame, driven by a crank, a vertical cog wheel, and a horizontal pinion, which grind very well, and are extremely ufeful. The ftones have a drum and hopper like a fingle mill. An eafy expeditious method of reducing grain to flour, is of the utmoft confequence to the inhabitants of every civilized country, and ingenious men deferve our beft thanks for their reiterated attempts to obtain fo defireable an end. Mill of Dumfriesfhire.

II. *Obfervations on the fuppofed Determination of the real Zero.* On the zero of heat.

Many eminent chemifts have endeavoured to difcover the real zero, or the divifion on the thermometric fcale correfponding to the total privation of caloric, but their labours have hitherto been unfuccefsful. Dr. Irvine of Glafgow contrived a theorem for this purpofe, which he founded on fuppofitions and experiments; and Mr. Dalton has lately propofed an hypothefis for the folution of the fame problem. Thefe may be confidered in their order. Dr. Irvine. Mr. Dalton.

Without giving Dr. Irvine's theorem, which is fo well known, I fhall infert the principal refults of the experiments and calculations founded on it.

In the following tabular view, the numbers in the firft column denote degrees of Reaumur's fcale *above* or *below* the freezing point, and thofe in the fecond column indicate degrees of Fahrenheit's fcale *above* or *below* the fame point, accordingly as the fign *plus* or *minus* is prefixed refpectively. Determinations of zero from Irvine's theorem.

	Reaumur.	Fahrenheit.
M M. Lavoifier and Laplace, from experiments on a mixture of nine parts of water and 16 parts of quick lime, place the real zero at - -	—1537°	=—3458$\frac{1}{4}$°
Their experiments on a mixture of fulphuric acid and water, in the proportion of 4 to 3 refpectively, fix it at	—3241	=—7292$\frac{1}{4}$
Their experiments on a mixture of the fame fluids in the proportion of 4 to 5 place it at - - - - -	—1169	=—2630$\frac{1}{4}$

And

	Reaumur.	Fahrenheit.
And the experiments of these celebrated philosophers, on a mixture of nitrous acid and quick lime in the ratio of $9\frac{1}{3}$ to 1, give for the real zero, $\frac{1889}{-0.01783}$ - - - - -	$+10594\frac{1}{2}$	$=+23837\frac{3}{5}$
Seguin, from the experiments of Lavoisier on the combustion of phosphorus narrated in his Elements of Chemistry, determines the place of the real zero to be - - - - - -	-842	$=-1894\frac{1}{2}$
He found, by the experiments of Lavoisier on the combustion of carbon, that it should be - - -	-1204	$=-2709$
And he inferred, from Lavoisier's experiments on the combustion of hidrogen gas, that the real zero should be at . - - - - -	-739	$=-1662\frac{3}{4}$
The comparison of the capacities of water and ice, which, according to the experiments of Kirwan, are as 1 to 0.9, fix the real zero at * - -	-600	$=-1350$
Dr. Crawford, from experiments on the combustion of hidrogen gas, places the real zero at † - - -	$-680\frac{8}{9}$	$=-1532$
Gadolin's experiments on the conversion of snow into water, taking the capacity of snow to that of water as 9 to 10 according to Magellan, place the real zero at ‡ - - - - -	$-649\frac{3}{5}$	$=-1461\frac{1}{5}$

These results differ widely, and one is impossible.

These ten widely different results, of which one is impossible, induce us to conclude that one of Dr. Irvine's suppositions at least is erroneous. Indeed, it seems to be agreed that his theorem is not well founded. The seventh and ninth numbers also shew, that a near approximation to accuracy is

* An interesting Paper by M. Seguin, from which the first eight numbers are taken, may be seen in the Annales de Chimie, V. 255.

† Crawford on Animal Heat, 267.

‡ Ibid. 458.

scarcely

ſcarcely attainable in theſe experiments. Let us now conſider the ſecond hypotheſis.

In the Philoſophical Journal, Vol. III. page 130, there is a very important paper of Mr. Dalton's on the Expanſion of Elaſtic Fluids, where he ſays, "In order to explain the manner in which elaſtic fluids expand by heat, let us aſſume an hypotheſis, that the repulſive force of each particle is exactly proportional to the whole quantity of heat combined with it, or in other words, to its temperature reckoned from the point of total privation: then, ſince the diameter of each particle's ſphere of influence is as the cube root of the ſpace occupied by the maſs, we ſhall have $\sqrt[3]{1000} : \sqrt[3]{1325}$ (10 : 11 nearly) : : the abſolute quantity of heat in air of 55°: the abſolute quantity in air of 212°. This gives the point of total privation of heat, or the abſolute cold, at 1547° below the point at which water freezes." Mr. Dalton's hypotheſis.

According to this hypotheſis, I have computed the following numbers, where the ſame is to be underſtood as in the laſt table, except the degrees in the firſt column, which are thoſe of the centigrade thermometer. Computations of zero made from the ſame.

	Celſius.	Fahrenheit.
$\sqrt[3]{1000} : \sqrt[3]{1325} :: 10 : 10.983146$, which places the real zero at -	—874°.12	=—1573°.43
$\sqrt[3]{1000} : \sqrt[3]{1375} :: 10 : 11.1199$, which fixes it at - - -	—892.93	=—1607.29
$\sqrt[3]{1167} : \sqrt[3]{1325} :: 10.528 : 10.9834$, nearly, which gives - - -	—952.34	=—1714.2
$\sqrt[3]{1000} : \sqrt[3]{1167} :: 10 : 10.528268$, and the real zero is at - -	—827.77	=—1485.98

Theſe deductions do not differ nearly ſo much as thoſe in the firſt table; yet they give grounds for believing that the ſuppoſition, or the greater part of the experiments is erroneous. It is no ſupport to the validity of this hypotheſis to ſay, that the firſt number nearly agrees with one deduced from a method totally different; for it might have been conſiderably greater or leſs than it is, and yet have been nearly equal to one of thoſe in the firſt table. There ſeems to be a ſlip in the degrees mentioned in Mr. Dalton's paper; for 167 + 158 = 325, the whole dilatation between 55° and 212°, while 77½° These reſults apparently of little value.

5 + 77½°

$+ 77\frac{1}{2}^{\circ} = 155^{\circ}$ only, which fhould be 157°, of which the half is $78\frac{1}{2}^{\circ}$. This might eafily happen: in the calculations of the third and fourth number I have ufed $78\frac{1}{2}^{\circ}$.

Mr. Dalton and Cit. Gay Luffac have confiderably improved the method of experimenting on gafes, and it is to be hoped they will pufh their improvements ftill further.

H.

VI.

Outline of the Craniognomic Syftem of Dr. Gall of Vienna. By Dr. Bojames.

(Concluded from Page 202.)

5. *Organ of the Inftinct of Sexual Union.*

5. Sexual union. THE organ of the inftinct of fexual union is fituated at the bafe of the *occipital*, behind the *medulla oblongata*, and the large aperture of the *occipital*.

This organ is not developed until the age of puberty; and it in a great meafure influences the figure of the back of the neck, becaufe the mufcles are attached to this part of the *cranium*.

The developement of this organ does not take place in animals which are caftrated before puberty, for which reafon it is invariably obferved that bulls have much ftronger necks than oxen, and "that horfes which have undergone this operation, before the neck is formed, are always flender in that part."

In the monkey, the hare and the cock, this organ is very diftinct, and in pigeons and fparrows the *occiput* forms a peculiar fack, which appears to be an appendage to the head; it is alfo known that thefe animals have a ftrong difpofition to copulate. The fame configuration is fometimes found in the cranium of men, and Gall poffeffes in his cabinet feveral fkulls of idiots,* who were noted for lafciviоufnefs, in which the *occiput* has a very unufual projection.

6. *Organ of the Mutual Love of Parents and Children.*

6. Parental and filial affection. The organ of the mutual love of parents and children occupies all the back and upper part of the *occipital;* from its pofition

tion it is intimately connected with the preceding organ, which confequently influences this by its action. "The exceffive developement of this organ fometimes contributes to form that prolongation of the *occiput* of which we fpoke in the preceding article."

This organ is in general more diftinct in women than in men, and throughout nature it is more defined in the female than the male; it is more particularly apparent in monkeys, whofe fondnefs for their young is fo remarkable as to have become proverbial.

"In general, all thofe animals which fhew much affection for their young are provided with it; and it appears to us that pigeons, the male of which fits on the eggs as well as the female, and which feed their young neftlings by a kind of rumination, may be taken as an example."

The cuckow, which never rears its young, is almoft entirely deftitute of this organ.

7. *Organ of Attachment, of Friendfhip.*

Behind and between the parietals, and on the lateral parts of the *occipital*, is placed the organ of attachment, or of friendfhip. 7. Attachment; friendfhip.

"By its pofition it has an intimate connection with the two preceding organs, and it is in animals deftined to live in focieties that the united action of thefe three organs takes place."

Dogs fhew the moft aftonifhing marks of attachment, and fpaniels, terriers, and houfe-dogs afford the greateft number of examples; thefe fpecies are alfo diftinguifhed by a large head, in which the developement of this organ is found behind and above the *zygomatic apophyfes*. The head of the greyhound, which is lefs fufceptible of attachment, is narrower behind, and ufually without this organ.

8. *Organ of Courage.*

The organ of courage is placed at the pofterior and inferior angle of the parietals. It affifts in increafing the fize of the head and feparating the ears from each other. Its proximity to the three preceding organs accounts for the fury of animals in rutting time, and for the extraordinary courage of thofe which have young, or which protect their females or the individuals of their fociety. 8. Courage.

 It

It is moſt diſtinct in the hyena, the lion, the wolf, ſome ſpecies of dogs, and particularly in the wild boar, the fierceneſs of which is well known.

On the contrary, the aſs, the greyhound, the ſheep, the hare, which are diſtinguiſhed by their timidity, are deſtitute of this organ; their heads are narrow behind, and their ears almoſt cloſe together.

A ſingular phenomenon ſeems to give countenance to the opinion of Gall reſpecting the ſituation of this organ; it is the involuntary action of a man who loſes his courage. He rubs behind his ears, as if endeavouring to ſtimulate the action of that organ from which this faculty is derived.

Note, "We have noticed an action of cats which appears to have ſome reſemblance to the above, and which regards the organ of attachment. It is, that in careſſing man they rub againſt him with the back part of the head."

9. *Organ of the Inſtinct to Aſſaſſinate.*

9. Aſſaſſination. The organ of the inſtinct to aſſaſſination is ſituated more forward than the organ of courage, towards the middle of the ſides of the parietals.

It is developed in all the carnivorous animals who live by prey; and Gall found it in the ſkulls of ſeveral murderers.

10. *Unknown Organs.*

10. Unknown. Two organs which correſpond with the temporal bones, are yet unknown as to their functions.

11. *Organ of Cunning.*

11. Cunning. The organ of cunning occupies the front and lower parts of the parietals; it is developed in all thoſe animals which are diſtinguiſhed for this faculty, ſuch are the fox, the pole-cat, the cat, the divér *, and it is in the moſt intimate combination with the *organ of theft*, which is only a prolongation of this towards the orbit, and is found in cats, ſome dogs, and in magpies.

* An obſervation which it appears difficult to reconcile to this, is that Gall conſtantly obſerved this organ to be developed in poets; he gives no explanation, but his obſervation is accurate.

It

It is perhaps to the developement of this organ that the Calmucks, whofe national character is that of theft, are indebted for that magnitude of their heads which has been noticed by fome obfervers.

12. *Organ of Circumfpection,*

The organ of circumfpection is fituated in the middle of the parietals, above the organ of cunning, and that of the inftinct to affaffination. 12. Circumfpection.

The exceffive developement of this produces irrefolution, and its defect caufes heedleffnefs; it is diftinct in the chamois and the rein-deer, the circumfpection of which is remarkable, and which do not tread over unknown paths but with the greateft precaution.

It is alfo found in fuch animals as only leave their retreats at night, fuch as owls, otters, &c.

13. *Organ of the Inftinct for Self-Elevation.*

This organ, in the middle of the inner border of the parietals, a little farther back than the middle of the upper part of the head, gives us a true notion of the difficulties to be overcome in the refearches of Gall, and at the fame time affords a ftriking example of the happy opinions of this accurate obferver. 13. Self-elevation.

He found this organ well developed in the chamois, and ftill more fo in the wild goat; he alfo noticed the fame in many men diftinguifhed by their pride. It was difficult to bring thefe obfervations into one point of view; but on confidering that the chamois frequents the moft lofty parts of mountains, that the wild goat is conftantly endeavouring to afcend higher, and that pride, attentively examined, is only the defire to be fuperior to others, he was perfuaded that this was the organ which produced thefe effects, *apparently fo differing,* and he took it for the organ of the inftinct of raifing or felf-elevation.

The head of the proud man, raifed and thrown back, tends to confirm this opinion ftill more,

Note, " It appears to us that the figure of a proud man, oppofed to that of a fubmiffive and modeft man, renders the truth of this notion more obvious. In the firft every thing is directed upwards; he fets up his hair, raifes his head, lifts his

his brows, turns up his eyes, throws back his ſhoulders, walks on tip-toe, and conſiders every ſurrounding object as beneath him; in the other, on the contrary, the hair falls naturally, the eyes, the eye-lids and the head are turned downwards, the body and the knees are ſlightly bent; in ſhort, every thing indicates a ſtate of ſubmiſſion, without a wiſh for ſuperiority."

14. *Organ of the Love of Glory.*

14. Love of glory.

When this organ is extended farther on the ſides, it forms that of the love of glory, a propenſity very analogous to pride.

15. *Organ of the Love of Truth.*

15. Love of truth.

The function of the organ which is ſeen at the poſterior and ſuperior angle of the parietals, is not exactly fixed by Gall; nevertheleſs he has reaſons to conſider this angle as the ſeat of the organ of the love of truth; but he has not yet collected a ſufficient number of facts to produce entire conviction.

Note, "We have ſome difficulty to perſuade ourſelves of the function attributed by Gall to this laſt organ; it appears to us, that an organ found among thoſe with which animals are provided as well as men, ought not to be appropriated to a faculty, like the attribute of veracity, is adapted only to the latter.

"Nevertheleſs this faculty, like that of pride, may be capable of great modifications in animals: and we acknowledge that we have known two men, one of whom was diſtinguiſhed by extreme veracity, and was furniſhed with this organ in a very eminent degree; but the other, on the contrary, whoſe diſpoſition for falſhood was extraordinary, was ſo entirely deſtitute of it, that inſtead of a projection, there was a cavity in this part of the head."

In the anterior, or lower part of the *os frontis,* Gall diſcovered many organs, the functions of which are of great importance.

At the commencement of his inveſtigations, he conſidered them as organs of the different ſpecies of memory; but afterwards obſerving that their action was not merely reproductive, but alſo productive, he was induced to conſider them as the organs of particular ſenſes, and to eſtabliſh, from this obſervation, the opinion that memory in general is only the reproductive operation of all the organs; and that imagination, on the contrary, is their productive action.

The

The automatic motion of man who endeavours to recollect, seems to relate to these organs. He places his hand, unconsciously, upon the lower part of his forehead. This action, though unobserved by him who performs it, is nevertheless constant, and is never confounded with that mentioned before, under the head of the organ of courage.

16. *Organ of the Sense of Locality.*

The organ of the sense of locality occupies the fore part of the *os frontis*, which corresponds to the protuberances above the orbits *(protuberantiæ supra orbitales)*; it is generally found in the craniums of those who are distinguished by large frontal sinuses, and uniformly have an anterior cavity adapted to an elevation of the brain. 16. Sense of locality.

When it acts reproductively, it constitutes what we term local memory *(memoria localis)*; by its productive operation, on the contrary, it inclines to combinations of new localities.

It is this organ which, in unknown places, directs the blood-hound, in whom it is strongly defined; it exists in all birds of passage; it incites them to change of place, to take long flights, and to return to their first habitations: the stork and the swallow are eminently provided with it, as well as those animals which remove the farthest from our climates. In such men as are furnished with it, we also discover a strong remembrance of places and a desire to travel; it is also constantly found in good painters of landscapes.

" A general, who regulates the dispositions of an army, and who at a glance must discern all the localities of the country he occupies, cannot succeed without it." The great Frederic furnishes us with a striking example. In advanced age, this organ is one of those which sensibly diminishes: it is also notorious that every kind of memory and of imagination grow weaker as the individual becomes older; the frontal sinuses then increase within; the action of the brain is no longer capable of obstructing their developement.

17. *Organ of the Sense of Things* (sensum rerum.)

The sense of things has its correspondent organ in the lowest and anterior part of the *os frontis*, between and higher than the preceding; its action is both productive and reproductive; and, in the latter case, it gives the remembrance of facts and things. 17. Sense of things

This

This organ is very neceſſary to education and inſtruction, which abſolutely require the recollection of paſt circumſtances; in age it is ſubject to the ſame change as the preceding.

Among animals, the elephant is particularly diſtinguiſhed by the developement of this organ; this animal alſo retains with the greateſt accuracy thoſe circumſtances and acts which have a reference to itſelf.

" Among men, we have found this organ, not only in thoſe whoſe memory of facts and objects was powerful, but alſo in thoſe which might be denominated ſyſtematic heads, from arranging facts in order, and hence forming concluſions; in thoſe who poſſeſs a happy conception, and are diſtinguiſhed by a deſire of univerſal knowledge; it even appears to us, that the operation of combining facts with a view to obtain a reſult, is one of the principal actions of this organ; at leaſt the elephant, who keeps his trunk filled with water, for the purpoſe of throwing it over the man who had offended him the night before, arranges a number of facts, and draws from them a reſult which is a true logical concluſion; and we are not acquainted with any other organ in the elephant to which this action can be referred.

" The automatic movement of the man who perceives that he has reaſoned wrong ſeems to give effect to theſe conjectures. He ſtrikes himſelf on the middle of the forehead."

18. *Organ of Painting, Senſe of Colours.*

18. Painting or colours.

The organ of the ſenſe of colours, or of painting, occupies the fore part of the *os frontis*, above the orbit; Gall has obſerved this organ in all painters of eminent talent.

" Having been acquainted with this diſcovery but a ſhort time, we have not been able to collect many obſervations on it; we have nevertheleſs remarked it in ſome individuals, and it is very apparent in the head of Raphael, in the National Muſeum, No. 57."

19. *Organ of the Senſe of Numbers.*

19. Senſe of numbers.

The organ which correſponds to the lower and exterior part of the *os frontis*, near the zygomatic apophyſis of that bone, poſſeſſes the function of the ſenſe of numbers; it exiſts in men whoſe memory is good with reſpect to numbers, and in arithmeticians who combine calculations with facility; it exiſts

exifts in a fpecies of magpie capable of counting as far as nine, which is the only example known among animals.

"We have had an opportunity of noticing this organ on the head of a blind man, at the Quinze-Vingts, remarkable for his arithmetical talents; and Gall preferves the bufts of many men which afford very inftructive examples."

20. *Organ of the Mufical Senfe.*

Above this organ is found that of mufical fenfe, or for founds. 20. Mufic.

It acts in a manner fimilar to the other organs, productively and reproductively; it gives the memory for founds; it facilitates the new combinations of mufical compofition; it induces birds to fing; it acts upon thofe who learn to fpeak, and in whom language is founded only upon this remembrance of founds.

It is intirely wanting in animals which have no mufical fenfe; it is ftrongly developed in the parrot and the ftarling; and thofe great muficians Gluck, Mozart, Haydn, Pleyel, furnifh us with ftriking examples.

21. *Organ of Senfe for Mechanics.*

In the lateral and inferior part of the *os frontis* is found the organ of fenfe for mechanics. The beaver which forms its habitation is eminently provided with it; it exifts in the field-moufe and in the birds which make their nefts with much art; it is met with in men of mechanical talents, who conftruct with eafe any machine, who ufe their hands with dexterity, and who excel in the different arts which require manual labour. Though it may be difficult to judge of the exiftence of this organ, when it is but flightly developed, "becaufe the temporo-maxillary mufcle covers this part of the cranium; it is neverthelefs very apparent if the faculty exift in a fuperior degree, and it is then one of thofe organs refpecting which there can be the leaft doubt. 21. Mechanics.

22. *Organ of Verbal Memory.*

In the interior of the orbit, at the bottom of the upper part, exifts the organ of verbal memory; it may be noticed from its developement by the effect it produces on the pofition of the ball of the eye, which it impels forwards, and more or lefs out of the orbit. 22. Verbal memory.

Perfons

Perfons provided with it eafily retain words by heart. Gall, while young, remarked this faculty in feveral of his fchool-fellows, who excelled merely by this talent, and who were diftinguifhed by the protuberance of their eyes. This was the firft obfervation which afterwards led him to thefe inveftigations. A number of fubfequent obfervations have eftablifhed the truth of its exiftence and of its function.

23. *Organ of Senfe for Languages.*

23. Languages. The organ at the exterior and upper part of the orbit, is called by Gall, the organ of fenfe for languages. Its prefence has a confiderable influence upon the pofition of the ball of the eye; it pufhes it downwards and towards the nofe, and increafes its diftance from the upper edge of the orbit; it never exifts in animals, in whom the ball of the eye is directed more towards the exterior fide of the orbit.

Diftinguifhed talents for the languages are invariably attended by its developement; it is eminent in great philologifts; and though it may be difficult to decide from external appearances, refpecting its exiftence, we have neverthelefs remarked that it never efcaped the penetrating eye of Gall, and that he was never miftaken in this point.

24. *Organ of the Memory of Perfons.*

24. Memory of perfons. The function of the organ at the upper internal part of the orbit, has not yet been difcovered by Gall; neverthelefs many obfervations on man and animals, fuch as the dog and the horfe, induced him to fuppofe it the organ of recollection of perfons. The developement of it, like that of the preceding, muft influence the pofition of the eye; it fhould contribute to remove it from the upper edge of the orbit, and to pufh it towards the external fide, if an equal developement of the preceding organ do not counterbalance its action.

25. *Organ of Liberality.*

25. Liberality. The organ of liberality is fituated in the anterior part of the *os frontis*, above that of the fenfe of locality, and of the fenfe for painting (Nos. 16 and 18), and befide that of the mufical fenfe (No. 20); its extreme developement is a concomitant of prodigality; it it not to be found in the mifer; in that cafe, a hollow is formed in this part of the *os frontis*. Gall is in poffeffion of numerous examples.

" The

"The proximity of the organ of mufic, and that of the fenfe for painting (Nos. 18 and 21) appear often to affift the developement of that of liberality; this is perhaps one reafon why we fo often find prodigality among men of eminent abilities in thefe fciences."

We conftantly obferve, that as a man becomes old, he becomes covetous; thus in advanced age the diminution of this organ is fo remarkable, that its place is frequently occupied by a cavity in the *os frontis*, which is fometimes of confiderable fize.

26. *Organ of the Power of comparing Things.*

The organ above the fenfe of things, in the middle of the forehead, is deftined to a faculty which Gall calls the fpirit of comparifon (*judicium comparativum*). 26. Comparing or combining.

It forms an oblong eminence, and is found in men who avail themfelves eafily of figures and images in converfation; who are not at a lofs for expreffions; who narrate fluently; and poffefs great eloquence.

27. *Organ of Metaphyfical Talent.*

When this organ is more developed towards the fides, fo as to form a round prominence, raifed in the middle of the forehead, it is the index of metaphyfical talent. Among the bufts of ancient philofophers, that of Socrates affords the moft ftriking example; among modern philofophers remarked for this organ, I fhall only notice Kant as one of the moft celebrated. 27. Metaphyfics.

Note, "I recollect in one of my firft fchool-fellows, to whom we had given the name of philofopher on account of his attachment to the abftract fciences; that his forehead prefented a very vifible developement of this organ."

28. *Organ of the Talent for Obfervation.*

The organ of the talent for obfervation fpreads over the whole of the anterior part of the *os frontis*, and its developement approaches more or lefs to the front of the vertical line. It is found on the craniums of all the men of obfervation in all ages; the celebrated phyfician Frank poffeffes it in an eminent degree, and Gall himfelf is very evidently furnifhed with it. 28. Obfervation.

29. *Organ*

29. *Organ of the Talent for Satire.*

29. Satire and wit.

The organ of ſatire and facetiouſneſs (*witz* of the Germans, *wit* of the Engliſh, *facetiae* of the Latins) correſponds with the frontal protuberance. Gall preſerves many examples of the truth of this opinion, and we have uniformly found it true.

30. *Organ of Goodneſs.*

30. Goodneſs.

The organ of goodneſs is in the middle of the forehead, above that of compariſon (No. 26.) It forms that oblong elevation which is conſtantly found in the portraits of Chriſt and of Mary, painted by Raphael and Correggio, and contributes much to that expreſſion of mildneſs and goodneſs with which we are delighted; it is always ſeen in the craniums of men naturally good, and is wanting in thoſe of the miſchievous and vindictive *.

Among animals the roe-buck, the hind, the pigeon, &c. are provided with it; on the contrary, animals of prey, ſuch as the eagle, the ſtarling, the tiger, the fox, &c. are without it; in the latter caſe, the *os frontis,* inſtead of being round and elevated, is depreſſed and hollow.

31. *Organ of Muſic or of Theatrical Talent.*

31. Theatrical repreſentation.

The very marked enlargement of the ſummit of the *os frontis,* is owing to the developement of the organ for the repreſentation of thoughts by actions, the organ of muſic, or theatrical talent.

" Gall has collected many obſervations to prove the truth of this opinion, nor will it be overlooked inattentively, conſidering the heads of the great performers at the different theatres of Paris."

Note, " We think we have alſo obſerved that this organ is particularly developed in the deaf and dumb; and we attribute it to the neceſſity ſuch perſons are under of acting continually, an exerciſe which neceſſarily facilitates its advancement."

* We are not here ſpeaking of that goodneſs which is the reſult of moral principles, that to which we allude exiſts as inſtinct, without being the produce of moral reflections.

32. *Organ*

32. *Organ of Theosophy.*

The organ of theosophy occupies the most elevated part of the *os frontis.* 32. Theosophy.

All the portraits of saints which have been preserved from former ages, afford very instructive examples, and if this character is wanting in any one of them, it will certainly be destitute of expression.

It is *excessively* developed in religious fanatics, and in men who have become recluse through superstition and religious notions.

It is the seat of this organ, which according to Gall, has determined men to consider their gods as above them in a more elevated part of the heavens. In fact, when we consider this subject philosophically, there is no more reason for supposing that God is placed above the world, than there is to suppose him below it.

33. *Organ of Perseverance.*

The last of the organs hitherto discovered by Gall, is that of perseverance, of constancy, of character; it is situated in the anterior and superior part of the parietals in the middle of the head; when it is in excess it causes obstinacy, but its defect produces inconstancy. 33. Perseverance.

"With regard to those parts of the skull in which Gall has not yet discovered organs, it is probable that his future investigations will afford him the means of success; on this subject, the work which he is about to publish, will give us more ample details. It will also be for him to convince us, by arguments perhaps incontestable, of the truth of his system, the exposition of which cannot be very satisfactory in a cursory outline."

We think it necessary also to remark, that the organs here enumerated are not distinctly perceptible, except in individuals who possess some faculty in an eminent degree, and that it is not possible to form a correct judgment of a moderate talent, on account of its organ being confounded among those which surround it. "We see no reason, philosophically speaking, for the calumnies which have been lately thrown upon the system of Gall, that *it tends directly to materialism.* When we admit of organs of the action of the internal faculties, the immeasurable space between mind and matter will continue

continue the ſame. Objects of a nature ſo unlike, are incapable of union. On the other hand, the will of man continues unimpaired; it is this which muſt counterbalance the operation of the organs, morality ought to ſubdue the paſſions."

BOJAMES, M. D.

VII.

Method of conveying Boats or Barges from a higher to a lower Level, and the contrary, on Canals, by means of a Plunger, inſtead of loſing Water by Locks. By Lawson Huddleston, *Eſq. of Shaftſbury, Dorſet. Communicated by the Inventor.*

Conſtruction of a lock for raiſing and lowering boats.

WHEREVER there is occaſion to convey a commercial boat or barge from a higher canal to a lower (uſually termed a higher or lower line) or *vice verſâ*, a lock muſt be conſtructed of ſtone or other fit materials in the ſpace between the higher and lower canal, ſo as to communicate at the ends with both of them. The dimenſions of the lock are to be as follow; its horizontal ſuperficies or area ſhould correſpond both in form and ſize with that of the boat itſelf, with the allowance of ſufficient room only for the boat to riſe and ſink freely within it; and its depth and height ſhould be ſuch that the water within it may riſe with a loaded boat floating therein from the level of the lower to that of the higher canal: there muſt alſo be two ſluices one at each end of the lock, large enough to admit the free ingreſs and egreſs of the boat, one of which muſt be accommodated to the level of the higher canal, the other to that of the lower.

Reſervoir on one ſide of the lock,

On one ſide of the lock there muſt be a reſervoir of equal height with that of the lock, the form of which may be rectangular or not (though the former may perhaps be the more convenient) the area of which ſhould be equal to, or rather in practice ſomewhat exceeding, that of the lock. In *this* caſe the perpendicular depth of the reſervoir ſhould be equal to double the intended riſe or fall of the veſſel within the lock, but where the areas of the lock and reſervoir are *unequal* this proportion varies: for as in all caſes the quantity of water to

 be

be difplaced out of the refervoir into the lock muft be equal in cubical content to that of the fpace within the lock between the furface of the lower line and that of the upper, it follows that if the area of the refervoir be greater than that of the lock the neceffary depth of the water to be difplaced will be fo much the lefs than in the cafe before ftated; and *e contra* if the area of the refervoir be lefs than that of the lock the neceffary depth of the water to be difplaced muft be fo much the greater: fo that whether this proportional quantity of water is to be obtained by the greater depth or the greater area of the refervoir, is a point for the confideration of the artift.

There muft be a communication fomewhere near the bottom between the lock and refervoir, that the water in each may be always on a level; which level may by means of the lower fluice always correfpond with that of the water in the lower canal, except during the actual operation of raifing or finking a boat within the lock. and communicating with it,

To the infide of the refervoir muft be fitted a * folid body or plunger, in fpecific gravity fomewhat exceeding water, and of fuch a bulk and form as will exactly fill the whole of the refervoir, allowing only fufficient room for the plunger to move freely up and down therein. in which a plunger may be raifed or depreffed.

Let us now fuppofe the refervoir to be 16 feet deep, and by means of the communication with the lower canal to be precifely *half* full of water. Let us alfo fuppofe the plunger to be fo fufpended by machinery as to be movable up and down within the refervoir, and to be barely above the furface of the water, when it, [the plunger] is at its greateft height. And now let us fuppofe that a boat or barge is floated through the lower fluice into the lock. That fluice being now fhut, as well as the upper one (which was all along fuppofed to be fo) the plunger is let down to the bottom of the refervoir: by this operation though the plunger be not in actual contact with the fides of the refervoir, much lefs what is called *water-tight*, (a neceffary circumftance in the cafe of forcing-pumps, piftons, &c.) the water in the refervoir will be forced into the lock, and thereby raife the boat or barge as much as the plunger finks, viz. 8 feet: in which fituation the water in the Method of operation: as the plunger fubfides the boat rifes and the contrary.

* At leaft fo far folid as to outweigh its bulk in water.

lock

lock being now on a level with that in the upper canal, the upper fluice may be raifed, and the boat floated out. The plunger being ftill *down*, if a boat be floated into the lock from the upper canal, let the upper fluice be fhut and the plunger be raifed, and the boat will in like manner defcend in the lock to a level with the lower canal.

Hydroftatic principle.

It is obvious that the *principle* of this invention is founded on this law in hydroftatics, viz. that two columns of water however different in lateral dimenfions, will, if there be a communication between them, always maintain one and the fame level.

The plunger is counterpoifed.

The plunger is to be counterpoifed by a weight acting on a fpiral curve (on the fame fhaft with the wheels or mechanifm that raife the plunger) in order to accommodate the action of the counterpoife to the *decreafing* weight of the plunger as it defcends into the refervoir; fo that being *counterpoifed* it may be eafily raifed or funk by means of the mechanical apparatus ufually employed for fuch purpofes, poffibly indeed this movement may alfo be effected by means of a fire-engine, but the *expediency* of fuch means is not meant to be fuggefted.

General remarks.

In theory the wall or partition common to both the lock and refervoir may not be neceffary, however ufeful it may be in practice.

Should occafion require it, the refervoir may be placed at any given diftance from the lock, provided there be a communication between them, at or near the bottom.

Without fuggefting any precife limitation (which can be learned only from experience) the projector conceives that locks on this conftruction are better adapted to low or moderate lifts than to high ones; boats alfo of a moderate fize (as recommended by Dr. Anderfon and Mr. Fulton) and fo conftructed as to exceed rather in *depth* than in length or breadth, feem beft adapted to the fcheme now propofed.

Advantages of this lock.

On a comparifon then between this lock and others of a late invention, it is prefumed that wherever it may be expedient to ufe it, it will be attended with the following material advantages.

No machinery except to the plunger;

1ft. The only machinery neceffary (the two fluices excepted) in this lock, is for the purpofe of raifing and finking the plunger or plungers.

2d. Whilft

2d. Whilſt the machinery in other contrivances above alluded to, is either wholly or in part liable to injury from water, the machinery in this (the two ſluices as before excepted) is wholly exempt from this diſadvantage. not liable to injury from the water;

3d. Whilſt caiſſoons, coffers, &c. require not only the opening of one or more ſluices, but alſo a previous fitting or adjuſting of their ends to the mouth of the canal to prevent the loſs of water, before the boat can enter the lock; according to this plan, on the opening of a ſingle ſluice the boat floats at once into the Lock without loſs of water, and without any ſuch delay or difficulty: when I ſay without loſs of water, it muſt be underſtood with this allowance, that if a boat goes down laden, and returns (or another goes up) unladen, the upper level gains a body of water equal to the weight of the cargo, *e contra* the lower canal gains ſimilarly if a boat goes down unladen and returns laden. nor requiring cloſe fittings. No loſs of water.

4th. If the water in the upper canal at any time be ever ſo abundant, no boat can paſs through the former locks but *by working the machinery*; whereas according to this plan the *machinery may be at reſt*, and the boats paſs on as through the common gate locks, whenever the upper canal can afford the neceſſary expence of water. If water be abundant, the machinery need not be uſed,

Laſtly. If at any time the machinery of any *one* lock of the former kind (out of the many that may be neceſſary in a given length of canal) ſhould happen to be diſordered, the *whole navigation is at a ſtand*, whereas in a lock of this conſtruction in caſe of ſuch an accident, the boats may ſtill paſs and repaſs as through a common gate lock. and therefore the navigation will not be impeded by any inevitable diſorder.

Let us ſuppoſe a boat (or barge) to be 20 feet long, and ſix feet wide, and to draw three feet water. The ſolid content will be 360 cubic feet, each of which weighing about 62 pounds, the burthen will be nearly 10 tons. Particular ſtatement of dimenſions, &c.

Is it propoſed to raiſe (or ſink) this boat eight feet, by means of a plunger of equal area or horizontal ſuperficies with that of the boat.

The deſcent of the plunger muſt in *this* caſe be equal to the aſcent of the boat, viz. eight feet, and the height of the plunger muſt (as in *all* caſes) be equal to the ſum of both, viz. 16 feet; the ſolid content of the plunger therefore will be 1,920 cubic feet, amounting in weight to nearly 53 tons.

Let

Mechanic force saved by enlarging the horizontal section of the plunger.

Let us now suppose that *two* such plungers are joined together laterally, so as to form a new plunger, of the same height, but of twice the area and weight of the former. It is plain that this new plunger (moving in a reservoir adapted to its dimensions) will by descending four feet, raise the boat in the lock as much as the other by descending eight feet, viz. eight feet:—and the momenta (consisting of weight multiplied by velocity) of both these plungers will be equal, because the product of 53 [tons] multiplied by eight [the velocity] equals the product of 106 [tons] multiplied by four [the velocity] each amounting to 424.

But it will be found that, as in *all* cases the height of the plunger is equal to the sum of its own descent added to that of the boat's ascent, the height of this new plunger, instead of amounting to 16 feet, will only amount to 12: its weight therefore will be only 79½ tons, and its momentum 318: it will therefore require so much the less time or labour to work it, in the ratio of 318 to 424, or three to four. Qu. Would not this objection arising from the necessity of having stronger machinery to support this latter plunger, be counterbalanced by the advantage of working it with so great a saving of labour and time?

Remedy for deposition of sediment.

Should it be apprehended that, notwithstanding the *forcing* effect of the plunger, the water in the reservoir may gradually deposit such a quantity of sediment at the bottom as to obstruct the descent of the plunger, it is obvious, that there may be a depending drain under the reservoir to be opened occasionally, or the bottom of the reservoir itself may be so much below the lowest point of the plunger's descent, as to render the cleansing it very seldom necessary *.

The machinery may be worked by waste water.

It remains to be observed, that as every canal ought to be so well provided with water as to allow of *some* waste (though not enough perhaps to supply the loss of a lock-fall with every boat that passes) the machinery of the lock may be worked thereby, without the mechanical aid commonly employed for such purposes; for if the plunger preponderates over the resisting medium, and counterpoise barely enough to overcome

* The impetus with which the water could rush out of the reservoir into the lock, by the action of the plunger, could probably raise and force out any sediment, except stones or gravel.

the

the friction of the axis, &c. it will at all times descend flowly within the refervoir, and raife the boat, when left at liberty fo to do; and if inftead of being allowed to defcend the whole eight feet required, it be ftopt after having defcended feven feet, the admiffion of one foot depth of water from the upper line into the lock, may be fufficient to raife the plunger again with the lofs of only $\frac{1}{8}$th part of a lock-fall of water.

Plate XII. Fig. 1. Exhibits a perfpective view of a model of the apparatus above defcribed, having part of the front taken away in order to fhew the interior parts. View of the apparatus.

A. Reprefents the upper fluice of the lock.

B. The lower fluice.

C. The plunger.

D. The fnail upon which the weight or chain is wound that carries the counterpoife.

VIII.

An eafy Method of Churning Butter. By CIT. JUMILHAC, *Prefident of the Society of Agriculture, of the Seine and the Oife**.

TO increafe the powers of man, is one of the attributes of mechanics, but in order that a mechanical contrivance may be truly ufeful, it is requifite that it it fhould be fimple and cheap. That which I now offer to the fociety, appears to me to unite thefe two advantages.

It is univerfally known, that in great heats and extremely cold weather it is difficult to churn butter. The labour of feveral perfons fucceffively, is often applied without any fuccefs, to procure the perfect coagulation of the cream. In vain during the rigour of winter, the cream is placed by the fire, or mixed with milk quite frefh and yet warm; and it is with as little fuccefs, that in fummer the churn ftaff is occafionally immerfed in cold water. All thefe means though of value, are infufficient if not feconded by celerity of motion in the act of churning.

Surprifed at the flownefs with which butter is formed, efpecially in winter, and after having continued the operation for

* Sonini's Bibliotheque Phyf. Oeconomique, No. 1.

five hours and a half, I thought I ſaw that the difficulty aroſe from the aukwardneſs and conſtraint of the arm that holds the churn ſtaff; both which are increaſed in proportion as the cream becomes thicker, as it is then neceſſary to make a greater exertion to raiſe and depreſs the churn ſtaff. This obſervation is ſo true, that a perſon who has churned butter for half an hour at the beginning, without experiencing any fatigue, cannot perform the ſame work for the ſpace of ten minutes after the cream has become thick and offers more reſiſtance.

Convinced that every interruption, or even diminution of ſpeed, are highly injurious to the butyraceous coagulation, I thought that, without altering either the churn or churn ſtaff, I could adopt a method which is already uſed in ſeveral other operations. Some members of the ſociety think they have already ſeen this application to the churn ſtaff, but not finding it mentioned in any book, I have preſumed, that the ſociety will not object to make it ſufficiently public, in order that it may be adopted in all dairies.

Nothing more is required than to fix to one of the beams of the ceiling, a pole three yards long, ſupported at the diſtance of two feet by a croſs piece, and itſelf faſtened at the end by two blocks or cleats nailed to the timbers. This pole may be about three inches or more in diameter at the upper part, and half that thickneſs at the lower or moveable end. At the end of the pole is tied a ſtrong cord, which is paſſed through a hole made near the top of the churn ſtaff, which is ſo faſtened and placed, that it continues ſuſpended in the middle of the churn.

When the churn ſtaff thus fitted up is preſſed downward, it enters eaſily into the cream; but the pole acting as a lever by its elaſticity, raiſes it immediately; this eaſes the arms, for the moment of the greateſt exertion is when you are obliged to raiſe them as high as the forehead to diſengage the churn ſtaff, and immediately force it down again.

Even if this method contributed only to diminiſh the principal part of the aukwardneſs and conſtraint, it would be a great advantage: but I ſhould think this but trifling and incompleat if the contraction of the arm were not remedied. After having conſidered what powers are jointly employed to produce the effect, I concluded that the poſition of the arm ought not to be neglected during this long operation; and ſeeking

feeking by trials for the leaft painful, I determined in favour of that in which the fore arm is employed in the whole motion.

I therefore perforated the handle of the churn ftaff about twelve inches below the hole through which the cord paffes that fufpends it to the pole, and I there faftened two fmall handles of wood of about two centimetres each, which are faftened together by an iron pin; fo that the handle of the churn ftaff refembles a crofs. The perfon who churns places his hands on the two handles, and by a fimple preffure, which is fo light, that two fingers are fufficient to force the churn ftaff to the bottom of the churn, he may churn for two hours without experiencing any real fatigue. For the motion of the elevation which alone demands the greateft exertion, is performed by the lever of the pole.

I made ufe of alder wood for the pole, as one of the moft elaftic and leaft likely to warp.

We fee from this defcription,

1. How little it cofts to eftablifh the mechanical help, which only confifts in a pole of alder wood of three yards long, and two fmall wooden handles adapted to the churn ftaff.

2. How greatly it affifts and increafes the power of the perfon who churns, by obviating the moft painful movement, by placing the hands naturally, fo as to require the motion of the fore arm only, which is evidently the leaft fatiguing.

I am convinced by the refult of conftant practice, that one third of the time is faved which is ufually employed in making butter.

IX.

Defcription of a Machine for rooting up the Stumps of Trees. By CIT. SAINT VICTOR, *Member of the Society of Agriculture, for the Department of the Seine**.

EVERY cultivator is well aware how detrimental the ftumps of trees are which remain in the ground, which inftead of being rooted up at the time they were cut down, have been fawed off clofe to the ftump, to fave the expence of digging them out of the earth.

Inconveniencies of leaving the ftumps of trees after felling the trunk.

* Bibliotheque Phyf. Oeconomique, de Sonini, No. 1.

Inftance.

In an eftate in Savoy, I was fubjected to thefe inconveniences, from the avarice of the proprietor who had felled a great number of large trees, and fawed them off clofe to the roots, thefe were oaks, walnuts, and chefnuts, which grew in the meadows and ploughed grounds, and he difpofed of them as timber in planks and fire wood, without taking any care to have them rooted up at the fame time, which would have been then more eafy, by the common practice of ufing the trunk as a lever.

Attempt to blow them up by gunpowder.

When I came into poffeffion of this eftate, I forefaw that I could by no means avoid rooting up a great number of ftumps that were very injurious to every kind of cultivation; and to avoid the long, troublefome, and expenfive labour of digging great trenches, which are ufually required to be made, in order to bring up the fort of roots, I thought I might employ the force of gunpowder. This attempt perfectly fucceeded, by means of a little machine of iron I had forged, of which I here join a plan, fection, and perfpective view, which fhews the manner of placing it beneath the ftump *.

Defcription of the Machine.

Machine for blowing up the roots of trees.

It confifts of a bar of forged iron, about two feet eight inches long, one inch thick towards the handle, and of two inches towards the breech or platform.

It is a fmall mortar with a plug and handle.

The platform which is circular, is 14 inches in diameter.

This platform ferves as the bafe of the chamber, or furnace of the mine, which is three inches in diameter, and three inches eight lines in the length of its bore.

* The ufe of this ingenious machine ought not to be confined merely to the ftumps of trees in our fields. Licentioufnefs and avarice within the laft five years, have deprived our country of a quantity of woods and thickets, and degraded by immenfe fellings, our nobleft forefts, the incalculable fource of riches for the prefent, and hope for future time. Whether thefe fpaces thus impoverifhed be converted into arable ground or pafture, or whether the imperious neceffity or claims of pofterity fhould require that they fhould again be planted with trees, the invention of Cit. Saint Victor, will be a moft ufeful application to eradicate from the ground thofe roots which would deprive us of a part of its product.

Note of the editor.

The

The ftopper or tampion which ferves as a plug to the mine, is of the fame diameter, to enter within after a flight paper or wadding. It is attached by a chain to the gun or mortar, which laft is eight inches in diameter.

About two inches above is added a fmall touch hole and pan. The hole is directed in an angle of 45 degrees, and is primed with powder to communicate with the charge with which the chamber is filled up to the ftopper.

This engine may be caft even with more facility in brafs or bronze, and in this cafe, it muft be a little thicker in all its dimenfions, in order to afford a refiftance equal to that of the forged iron.

Ufe of the Machine.

When the machine is charged with powder, a fmall excavation is made with a pick axe, in the center of the ftump. The machine is then placed in it, fo that the plug immediately touches the wood. Care muft be taken to fill all the vacancies, either with ftones, or pieces of iron, or wood, more efpecially beneath the platform of the machine, in order that the explofion of the powder may have its full effect on the ftump, of which if neceffary, the principal roots fhould firft be cut, if any appear on the furface of the ground near the ftump, that is to be eradicated.

Application beneath the ftump intended to be blown up.

When the machine is firmly fixed in its place, the priming is put into the pan, a flow match applied, the length of which is fufficient to allow time to retire to a proper diftance from the explofion.

By infpection of the plan and fection, every one will perceive the utility of this fimple machine, and it may eafily be made by any intelligent fmith.

Delineation and plate.

A. Plate XIII. Fig. 1. Plan of the machine of about two feet three inches long.

B. Plan of the machine, 10 inches high, comprehending the plug.

a. The plug with its cap faftened to the chain.

b. The chamber for the powder.

C. The touch hole.

The middle figure reprefents the machine placed under the ftump of a tree *.

* Mr. Knight, ironmonger, of Fofter Lane, has contrived a fimple apparatus for fplitting blocks of wood with gunpowder, of which the defcription will be given in our next.—N.

Method

X.

Method of Secret Writing, by means of a Steganographic Scale.
By J. B. Berard *.

Methods of secret writing.

AN infinite variety of methods of fteganography or indecipherable writing has been devifed; of thefe, fome are defective in theory, and others inconvenient in practice. It is not my intention to difcufs the advantages or inconveniences of all thofe which are known, it would lead me too far. I fhall therefore confine myfelf to noticing a few of the principal ones.

Alphabets of convention.

Alphabets of convention afford but little fecurity; the characters which perform the functions of vowels and confonants, recur in fuch fituations, as to make their ufe very apparent; and it is afcertained, that by patience and a little fkill the fecret is eafily difcovered.

Sympathetic ink.

The fympathetic inks, feven in number, are not more fafe, becaufe after feveral trials, that re-agent which renders the writing legible, is at length difcovered.

Folding the paper.

Thofe methods which depend on folding the paper in a particular manner, as was practifed by the Spartans, are inconvenient, and afford but little defence againft curiofity.

Kircher's method.

The method of Kircher, though fufficiently certain in its principle, is of little value in practice; befides which, its performance is both tedious and inconvenient: a point wrong placed or omitted, is fufficient to render the fecret unintelligible to the correfpondent.

General enumeration.

In the fixth number of the *Journal de l'Ecole Polytechnique*, p. 382, is a table, in which Cit. Haffenfratz has claffed all the modes of correfpondence, whether by writing or otherwife. That which I have devifed, is as follows; it will perhaps be found to poffefs the double merit of fimplicity aud fecurity †.

* Melanges Phyfico-Mathematique.

† This method has been made ufe of for the private correfpondence of Cit. Forfait, minifter of the marine, and the colonies, and prefident of the lyceum of arts. There is no inconvenience in mentioning the fact; for the advantage of a good method confifts in

Take a rule of pasteboard, wood, ivory, or copper, about one inch broad and seven inches long, divide its edge into 30 equal parts, and between the divisions write the first thirty natural numbers, from 1 to 30 inclusive, in any arbitrary order. The following is the manner of using this rule. New method by a scale of transposition.

Operation for writing. First make a minute or outline of the secret you wish to send, then place the rule or steganographic scale on the paper intended for the secret, and mark the two ends of the rule by two small lines of ink, the reason of which will be seen below. The process. To write.

This being done, transcribe the first thirty letters of the minute, writing the first letter opposite to the figure 1, on the rule, the second opposite to the figure 2, the third opposite to the figure 3, and so on to the end.

Bring down the rule and transcribe in a similar manner, opposite to the figures, the next thirty letters of the secret, that is to say, from the thirty-first to the sixtieth inclusive.

Continue thus transcribing in new lines, each consisting of thirty letters, until the whole secret is written.

The punctuation must be carefully inserted to the right of that letter which is on its left in the minute, and a mark like this + must be placed above the last letter of each word, to distinguish it from that which follows, and thus render the reading easy to the correspondent.

Operation of the correspondent. The correspondent who receives the secret with the letters thus misplaced, will be able to transcribe it, and replace the letters in their proper order, by inversing the preceding operation. To decypher.

For this purpose, he must be provided with a rule similar to that made use of for deranging the letters of the secret. He will place it on the first line, so that the letters correspond with the figures on the rule. This will be easily accomplished by means of the two lines of ink which mark the ends of the rule.

It will then be easy for him to transcribe the letters of the secret in their proper order, beginning with that opposite to

in its capability of being employed by all the world, without diminishing its secrecy for each individual; it will be seen that, in this plan, it is the secret of the series agreed on, and not that of the method, which renders the correspondence safe.

figure

figure 1, next that oppofite to figure 2, and fo on to thirty he will then bring down the rule, and proceed in the fame manner with the fucceeding lines.

Example.

Example. Suppofe this to be the fecret to be fent.

L'efcadre mettra à la voile au premier vent favorable; elle fe rendra à Toulon, où je lui enverrai des ordres ultérieurs.

Suppofe the feries of the fcale employed fhould be this,

2. 4. 6. 8. 10. 12. 14. 16. 18. 20. 22. 24. 26. 28. 30. 1. 3. 5. 7. 9. 11. 13. 15. 17. 19. 21. 23. 25. 27. 29.

The anagrammatic writing or tranfpofition of the fecret, will be;

```
    +    +      + +                + +
lecde e ta lv i e u r meL's a r mtrà ' ol ape il
         +   + +     +      +               +
lvnfv r be; le e e d a Ture t a oale l sr nrà o'l
      +     +             +  +  +         +   +
loojl i ne ri c o'd e ut ln, ù e uevr a ds rrs ll
lrer                     é i u s.               l
```

Obfervations.

The combinations to be decyphered in one fingle line, are many millions of millions,

1. If we wifh to form a notion of the fecurity of this method, let us confider what a geometer muft do who undertakes to explain the fecret.

Every line contains a number of combinations, expreffed by the product of the numbers 1. 2. 3. . . . . to 30. that is to fay, a number expreffed by 265 followed by 30 cyphers. This number of anagrams is equal to that of the grains of fand which would encircle the whole world, fuppofing each grain to be lefs than one hundredth of an inch in diameter. This, without doubt, is more than fufficient to difcourage the moft perfevering Newton, and it muft be acknowledged that the difficulty may be confidered as an abfolute impoffibility.

and they are ftill more taken all together.

But this is not all, after the labour of a multitude of ages, this geometer would be no farther advanced; for among the infinity of combinations produced from the thirty letters of one line of the fecret, he would find an innumerable multitude that might yield a rational meaning, and by what means would he be able to difcover which of them the author of the fecret had in view? In fhort, what muft be the difficulty where it becomes neceffary thus to combine all the lines of the fecret at the fame time? Here the imagination is loft.

This method is adapted to numerous correfpondents.

2. In the event of many correfpondents, it would be eafy for the greater fecurity to agree upon a different feries with each, if the fcale is made of pafteboard, it may be marked with

with four feries, and may ferve for four correfpondents: if it be of ivory, the feries may be changed at pleafure, by writing the figures with a pencil: the fcale may even be compofed of thirty fmall detached moveable pieces of brafs flipped upon a flat iron rod, and fecured with a nut at one of its ends.

3. It may be eafily conceived, that the principles of permutation may be varied to infinity, for inftance, by changing only the order of the fyllables, the words, or the lines. The above method on which I have fixed, appears to be at once eafy, certain, and concife, and to require the fimpleft apparatus. Other permutations.

4. If it be defired to conceal even the appearance of a fecret, the method above defcribed may be employed to write with one of the fympathetic inks, at the end of an oftenfible fubject.

5. *Another method.* Having divided the paper into 100 or 200 fquares, write in each a letter, a fyllable, or a word of the fecret, and alfo one of the numbers of the concerted feries: feparate and mix the fquares; the correfpondent will only be required to replace them in their order by means of the figures of the feries. Another method by the fcale,

For the greater fecurity, the edges of the fquares fhould be pared to deftroy the connection of the adjoining fides.

6. *Method of intercalations.* Draw on pafteboard, fome parallel divifions about half an inch afunder, pierce them with holes, either equal or unequal, of an arbitrary fize, and at any diftance from each other: lay the pafteboard on the paper, and write the fecret by inferting fucceffively from left to right, a letter, fyllable, or word in each vacancy: remove the pafte board and fill up the intervals with infignificant letters; or it will be better, if time permit, that the letters fhould form with thofe of the fecret, a reafonable meaning, not liable to be fufpected. another by intercalation.

The correfpondent will eafily read the fecret through holes in a fimilar pafteboard.

The infertion of unmeaning letters may be avoided by ufing three or four pafteboards of equal fize, but fo pierced, that when they are laid on each other, all the holes will be clofed. By writing through each of thefe in fucceffion, all the lines will be filled. The correfpondent muft be provided with fimilar pafteboards, and will read the fecret through them by applying them fucceffively to the writing.

This

This method is shorter than that of the anagrammatic scale, and is preferable when there is much to write; but it is less secure, and requires a more complicated apparatus.

XI.

Description of a Magazine Pistol, which when loaded is capable of being discharged Nine successive Times through the same Barrel. W. N.

Account of a magazine pistol.

I AM indebted to the liberality of the Right Honourable Lord Camelford for the accurate drawings of the curious and valuable piece which forms the subject of the present Memoir, as well as for permission to use and examine it in any manner I might think proper. It is of German make; the workmanship very good, and it has been used by his Lordship without any particular care in various parts of the world. Its construction does, in effect, shew that its use is attended with neither danger nor uncertainty; but I shall postpone my remarks till I have given the description.

Description by reference to the drawings.

Figs. 1 and 4 in the Plates XIV. and XV. exhibit views of the two sides of the pistol. Fig. 5. is a section to shew the magazines, and Figs. 2. and 3. shew what may be called the chamber piece. The large face of this last (Fig. 2.) is slightly taper or conical from X towards Y, where the diameter is smallest, and the small part B is cylindrical, excepting that a portion is scooped out on one side marked by the dark space D in Fig. 3. Its proper place in the pistol is in the breech, crossing the line of direction at right angles. When the lock is off, it goes into its cell from the side Fig. 1. which it fits with considerable accuracy, but does not come to rub hard, because the Plate X forms a projecting face which stops it. In this situation its small cylindrical part B projects outwards, and is received in an hole of the same diameter in that part of the lock beneath the hammer where the pan is usually placed. In fact, the cavity D, Fig. 3. does constitute the pan when its position is such as to be immediately beneath the covering face of the hammer. The screw B which goes into the cylindrical piece, and Z, which goes into the stock, are the fastenings by which the lock is secured. At the opposite end

A,

A, Fig. 2. the chamber piece terminates in a ſquare, upon which the broad head of the lever L, Fig. 4. is fitted, and kept down by the ſcrew A, which goes into the chamber piece. It is to be underſtood that the cock and hammer are conſtructed, and act in the ſame manner as in the beſt fire arms, and do not therefore require to be deſcribed. The lever is capable of being moved from the poſition M to thoſe of L and N; but is prevented from deſcribing the remaining part of the circle by an interior ſtop, which may be eaſily imagined without any attempt at minute explanation.

Deſcription of a piſtol which fires nine balls in ſucceſſion by once charging.

Fig. 5. being a ſection through the middle of the ſtock, breech, and part of the barrel, ſhews the relative ſituation of the magazines, with the chamber piece, and other parts. The balls S, 2, 3, 4, &c. are lodged in their proper receptacle, being put in through the hole R, Fig. 4. and the powder is lodged in its magazine Q Q. both which are cloſed by the door T, which is then faſtened by a ſmall bolt and back ſpring. A, Fig. 5. ſhews the ſituation of the chamber piece at the time when the lever is brought to the poſition M. This is done with the muzzle of the piece pointing towards the ground, and the effect is, that powder runs into the chamber P (ſee alſo Figs. 2 and 3) and a ball into the chamber S. The lever is then moved from M to L and N, by which proceſs the ball chamber drops its ball into the barrel as it paſſes oppoſite to N, where the ball remains, becauſe the actual bore of the ſcrew barrel is not wide enough to allow it to paſs farther than juſt to clear the moveable chamber. As ſoon as the lever has arrived at the poſition N, the powder chamber P is exactly oppoſite the ball, and ready to be diſcharged againſt it. After the diſcharge the muzzle is to be again depreſſed, and the lever moved from N to M: the chambers become again charged with powder and ball; and by returning the lever back to N, this ball and powder become duly placed for a ſecond diſcharge. It is obvious that theſe diſcharges may be repeated until all the balls have been fired out. The ſmall bridge in the powder chamber P (Fig. 2.) is to prevent any impediment from the entrance of part of one of the balls into the receptacle, and the perforations W, W, in the breech, ſerve to clear the ſurface of the chamber piece from any foulneſs it might acquire.

Thus

Description of a pistol which fires nine balls in succession by once charging.

Thus far we have ſpoken only of loading and diſcharging; but this piece would admit of little rapidity of effect, if it did not at the ſame time cock and prime itſelf. It may be obſerved, that the projecting part, or ſtud C, is fixed to the chamber piece Fig. 2. and 3. a very little behind the ſhot chamber; ſo that it ſtands at the top of the lock at the time of charging. A thin flat bar of iron proceeds from the cock, and ſlides with it along the face of the lock plate, withinſide the hammer: againſt this plate the ſtud C acts, and brings it to full cock a little before the lever arrives at the poſition M; at the ſame time that the more prominent part of C preſſes on the back of the hammer, and ſhuts the pan. The lock is therefore put into the condition to give fire by the ſame ſimple operation, and preciſely at the ſame moment as the charge is taken from the magazines.

It now remains only to be ſhewn how the priming is given. The excavation D in the ſide of the ſmall cylinder B, Fig. 3. conſtitutes the pan, and the ſmall dot repreſents the touch hole paſſing through the metal into P the powder chamber. When the lever is in the poſition M, Fig. 4, the cavity D is exactly placed beneath the covering face of the hammer G: but at the time of charging it has the poſition Fig. 3. conſidered with regard to that of Fig. 1. In this laſt figure the dark ſhaded ſpace H denotes a reſervoir for priming, the door of which may be opened and ſhut under the action of an ingenious back ſpring, operating nearly like that of a claſp knife. A long perforation or ſlit communicates from this reſervoir to the ſpace in which the cylinder B revolves: ſo that when the excavation D paſſes that ſlit (during the return of the lever) it becomes filled with powder, which it carries round to the laſt poſition, which is exactly that in which it muſt receive the fire whenever the piſtol is to be diſcharged.

The hiſtory of this conſtruction ſeems to be imperfect; but there is reaſon to ſuppoſe that it is of ſome antiquity. The magazine air gun of *Colbe* conſtructed for carrying ten balls, and lodging them ſucceſſively in the barrel by a croſs cylindrical piece was made early in the laſt century, and is deſcribed in Deſagulier's Lectures, and moſt elementary books. In the preſent arm the contrivances are highly judicious, as well with regard to mechaniſm as to arrangement. If it were poſſible for the powder magazine to be ſet on fire at the diſtance

tance of a femi-circumference of the chamber piece from the explofion, the only effect would be that the door would be blown open, and this is fituated in a place where it could do no harm. The fame remark is alfo applicable to the magazine for priming.

Defcription of a piftol which fires nine balls in fucceffion by once charging.

How great the advantages muft be in battle, for a man to be able to reload his piece by a fimple movement of one fecond of time, without taking his eye off his enemy; or how confiderably ufeful this invention might prove in the defence againft robbers need not be ftated. It can indeed be ftated, that the opponent may alfo provide himfelf with the like advantage; and then we have only to urge the argument, that the duration of wars have been diminifhed, and its humanity diminifhed by rendering the means of annoyance more perfect.

I have difcharged this courfe of balls feveral times, and I find that the whole nine balls can be fired in 30 feconds.

XII.

On the Diffemination of Plants. *By* CIT. L. REYNIER *.

THE hiftory of vegetables affords fome facts to which I think it my duty to call the attention of naturalifts: they relate to the diffemination of plants, and to the means by which this is effected. I have already collected feveral obfervations on this fubject in the dictionary of Agriculture of *L'Encyclopédie Méthodique*, article *Diffemination.*

Diffemination of plants,

There are two natural means of reproduction: one of thefe is by the roots, which, fpreading outwards, form new ftems round the mother plant; this reproduction is flow, and can only take place gradually and without intervals; the other is by the feeds, which being carried by the winds, or by the hooks with which they are provided, or by animals which fwallow them, and afterwards depofit them, unchanged, in their excrements, are removed to greater diftances, though ftill within a limited circle. It is not therefore wonderful, to fee a plant fpring up in a fpot, where the fame fpecies is known to exift at no great diftance; its introduction is in the clafs of poffibilities.

by the fpreading of the roots, or tranfportation of feeds.

* Decade Philofophique, No. 13. An. XI.

But

New vegetations that appear when the face of the ground is disturbed.

But that which ought to excite our attention, is the production of a new vegetation in a place which has undergone ſome very great changes. In all caſes when the ſurface of a place has been altered either by the falling or removal of the earth, by opening roads through foreſts, or by draining marſhes, plants are ſure to be found in the following year, which did not exiſt there before, while the former ſpecies diſappear, except a few coſmopolites, if the expreſſion may be allowed, which, though they re-appear, undergo a remarkable alteration *(Dict. d'Agr. de L'Encycl. meth.* art. *Climat.)* When a wood is grubbed up, the foreſt plants ceaſe to grow, and thoſe natural to tilled ground immediately germinate. Labat notices (ſee Vol. I. p. 386.) that, in his time, at the Antilles, as ſoon as the ground was cleared, its ſurface became covered with purſlane. In thoſe places where charcoal is made, in foreſts at a great diſtance from any habitation, I daily obſerve plants, different from thoſe which were formerly there, and which are natives either of dry paſtures or cultivated lands, ſuch as the (vergerette) of Canada, the annual veronicas, &c. Forſter, in his voyages with Cook, took notice of ſeveral iſlets in the middle of the ſea, ſimply rocks of coral, on which the firſt rudiments of vegetation were forming. In the low valleys of the downs of Holland, I have gathered a ſatyrion and an ophrys, natives of the ſummits of the Alps and the plains of Spitzbergen *(Satyrium viride. L. Ophrys monorchis, L.)* But theſe downs, according to the obſervations of Cit. Décandolle, *(Ann, d'Agr. T. XIII. p. 372.)* are not of very ancient origin. How is the introduction of theſe two plants, which are naturally provided with very limited means of multiplication, to be accounted for? I have expreſſed a wiſh in the Encyclopedie, that the new iſlands formed by volcanos, and particularly thoſe near Santorini ſhould be examined. One of our moſt accurate obſervers is on the point of viſiting the latter, *(See d'Olivier, T. II. p. 238)*. The new one produced by the eruption of 1707 to 1711, does not yet ſhew any appearance of vegetation, the air is ſtill mephitic; that which aroſe in 1573 has ſome traces of vegetation, particularly ſome graſſes, and a ſmall fig tree; the latter may have been depoſited by ſome bird, but by what means were the graſſes conveyed?

Are theſe produced by the roots or ſeeds of other plants.

It is true that seeds which are buried to a certain depth will be preserved for a great length of time, and being afterwards submitted by culture to the action of the light and water, will germinate. But is it to be presumed, that the seeds of purslane were preserved in the earth of the Antilles, during the many ages the forests had endured before they were destroyed? Is it probable that, in the forests of Gaul, whose antiquity is well ascertained, the seeds of those plants, which appeared after they were rooted up, or after placing a charcoal furnace in them, should have been preserved sound from before the existence of the forests? The dissemination of plants has necessary limits; beyond these, we must have recourse to other explanations.

Could the seeds have been so long preserved.

Lastly, the plants of the class cryptogamia, and particularly fungi, whose form is constantly determined by the nature of the substance on which they grow, and is constantly the same in similar circumstances, although they do not appear at stated seasons like other vegetables, also present new facts. It is not long since Cit. Ventenat observed a boleta of a peculiar new shape, which was produced on a human body. I have noticed the clavaria, which is constantly formed on the crysalides of caterpillars (*Journal de Phys. Année.* 1787); others grow on those fruits which have a woody covering; such as are formed on the fragments of fir, are not the same as those which grow on the wood of the oak. The husk of the nutmeg produces a fungus peculiar to itself, (*Ancienne Encycl.* art. *Muscade*). Lastly, the fungi which are formed on the wooden props of mines, cellars, &c. are not the same as those produced by the same woods when they decay in the open air. It is difficult to conceive the dissemination of seeds from one mine to another, particularly to those which are opened in situations where no mines had existed before; nevertheless I have found all those which I have examined in different countries to possess very nearly the same form.

Facts respecting the cryptogamia.

All these considerations may induce us to presume, that nature is daily exercising the same powers which she possessed at the creation, and it would be interesting to examine the causes and ascertain the means of execution; but this can only be the consequence of long and diversified investigations, carried on by a great number of observers.

It is presumed that plants continue to be formed without propagation.

XIII.

An Essay on the Declivities of Mountains. By RICHARD KIRWAN, *Esq. L. L. D. F. R. S. and P. R. I. A,*

Introductory remarks.

AMONG the various causes to whose activity the planet we inhabit owes its present wonderfully diversified appearance, some undoubtedly exerted their influence from its very origin, and others at subsequent periods; of these last one at least, namely, the Noachian deluge, was universal in its operation; while the effects of many more were partial and local, such as those resulting from earthquakes, volcanos, particular inundations, &c.

In a general survey of the globe, it is only to general causes, whose operation was universal, that our attention can be directed; the effects of partial causes being the proper objects of the geological history of those countries that were particularly affected by them.

But to distinguish causes of the former class from those whose operation was more confined, it is necessary to discover some character by which their effects may unequivocally be discerned.

Now a general *uniformity*, or agreement in some particular circumstance in every part of the globe, seems to be a sure test of the operation of some general cause. The discovery of uniform appearances is therefore of primary importance in geological researches. In the present essay I shall confine myself to the investigation of one instance of this sort, namely, the *inequality of declivity* which the sides or flanks of mountains exhibit in every part of the globe hitherto examined according to the points of the compass to which they face, and are exposed.

All high mountains or hills have one side much steeper than the other.

That one part of almost every high mountain or hill is steeper than another, could not have escaped the notice of any person who had traversed such mountains; but that Nature in the formation of such declivities had any regard to different aspects or points of the compass, seems to have been first remarked by the celebrated Swedish geologist Mr. Tilas, in the 22d vol. of the Memoirs

Memoirs of Stockholm for 1760 *. Neither Varenius, Lulolph nor Buffon in his Natural Hiftory publifhed in 1748, have noticed this remarkable circumftance.

The obfervation of Tilas however relates only to the extreme ends, and not to the flanks of mountains; with refpect to the former, he remarked that the *fteepeft* declivity always faces that part of the country where the land lies loweft, and the *gentleft* that part of the country where the land lies higheft, and that in the fouthern and eaftern parts of Sweden they confequently face the E. and S. E. and in the northern the W. The effential part of this obfervation extends therefore only to the general elevation or depreffion of the country, and not to the bearings of thefe declivities.

The steep fide faces the low country. Tilas.

The difcovery that the different declivities of the flanks of mountains bear an invariable relation to their different afpects, feems to have been firft publifhed by Mr. Bergman in his phyfical defcription of the earth, of which the fecond edition appeared in 1773. He there remarked that in mountains that extend from N. to S. the weftern flank is the *fteepeft*, and the eaftern the *gentleft*. And that in mountains which run E. and W. the fouthern declivity is the fteepeft and the northern the gentleft, vol. 2d. § 187.

The weftern fide is fteepeft. Bergmann.

This affertion he grounds on the obfervations related in his firft vol. § 32, namely, that 1° in Scandinavia the Suevoberg mountains that run N. and S. feparating Sweden from Norway, the weftern or Norwegian fides are the fteepeft, and the eaftern or Swedifh the moft moderate, the verticality or fteepnefs of the former being to that of the latter as 40 or 50 to 4 or 2 †.

Inftances. In Scandinavia.

2dly. That the Alps are fteeper on their weftern and fouthern fides than on the eaftern and northern.

The Alps.

3dly. That in America the Cordelieres are fteeper on the weftern fide, which faces the Pacific Ocean, than on the eaftern. But he does not notice a few exceptions to this rule in particular cafes which will hereafter be mentioned.

The Cordelieres.

Buffon, in the firft vol. of his Epochs of Nature publifhed in 1778, p. 185, is the next who notices the general prevalence

Remarks of Buffon,

* See alfo vol. 25, Swed. Abhandl. p. 291, where *Cronfted* explains fome obfcure parts of Tilas's obfervation.

† The verticality of the fides is inverfely as the length of the defcent.

of this phænomenon, as far as relates to the eaftern and weftern fides of the mountains that extend from north to fouth, but he is filent with refpect to the north and fouth fides of the mountains that run from eaft to weft; nay, he does not feem to have had a juft comprehenfion of this phenomenon, for he confiders it conjointly with the general dip of the regions in which thefe mountains exift. Thus he tells us, vol. 1ft, p. 185, that in all continents the general declivity, taking it from the fummit of mountains, is always more rapid on the weftern than on the eaftern fide; thus the fummit of the chain of the Cordelieres is much nearer to the weftern fhores than to the eaftern; the chain which divides the whole length of Africa, from the Cape of Good Hope to the mountains of the Moon, is nearer, he fays, to the weftern than to the eaftern feas; of this however he muft have been ignorant, as that tract of country is ftill unknown.

The mountains which run from Cape Comorin through the peninfula of India are, he fays, much nearer to the fea on the eaft than on the weft; he probably meant the contrary, as the fact is evidently fo, and fo he ftates it in the 2d vol. p. 295; the fame he tells us may be obferved in iflands and peninfulas, and in mountains.

and other authors;

This remarkable circumftance of mountains was notwithftanding fo little noticed, that in 1792 the author of an excellent account of the territory of Carlfbad in Bohemia, tells us he had made an obfervation, which he had never met with in any phyfical defcription of the earth, namely, that the fouthern declivity of all mountains was much fteeper than the northern, which he proves by inftancing the *Erzgebirge* of Saxony, the Pyrenees, the mountains of Switzerland, Savoy, Carinthia, Tyrole, Moravia, the Carpathian and Mount Hæmus in Turkey, 2, Bergm. Jour. 1792, p. 385, in the note.

Herman;

Herman in his Geology, publifhed in 1787, p. 90, has at leaft partially mentioned this circumftance, for he fays that the eaftern declivities of all mountains are much gentler and more thickly covered with fecondary ftrata, and to a greater height, than the weftern flanks, which he inftances in the Swedifh and Norwegian mountains, the Alps, the Caucafian, the Appenine and Ouralian mountains; but the declivities bearing a fouthern or northern afpect he does not mention.

La Metherie;

La Metherie, in the 4th vol. of his Theory of the Earth, of which the fecond edition appeared in 1797, a work which

4 abounds

abounds in excellent obſervations, p. 381 *, produces numerous inſtances of the inequality of the eaſtern and weſtern declivities, but ſcarce any of the northern and ſouthern, whoſe difference he does not ſeem to have noticed, but he makes a remark which I have not ſeen elſewhere, that the coaſts of different countries preſent ſimilar declivities.

With regard to eaſtern and weſtern aſpects, he thinks that a different law has obtained in Africa from that which has been obſerved in other countries, for in that vaſt peninſula he imagines the eaſtern declivities of mountains are the ſteepeſt, and the weſtern the gentleſt. Of this however he adduces no other proof, but that the greateſt rivers are found on the weſtern ſide: this proof ſeems inſufficient, as, if mountains be ſituated far inland, great rivers may flow indiſcriminately from any ſide of them, and ſometimes few rivers flow even from the ſide whoſe deſcent is moſt moderate, for inſtance, from the eaſtern ſide of the mountains of Syria; the Elbe and the Oder, two of the greateſt rivers in Germany, take their courſe from the weſtern ſides, the firſt of the Bohemian and the other of the Moravian mountains, which yet are the ſteepeſt. Many originate from lakes, as the Shannon with us; many take ſuch a winding courſe, that from a bare knowledge of the place of their diſemboguement it is impoſſible to judge from what ſide of a mountain they iſſue, if from any; their courſe at moſt diſcovers the depreſſion of the general level of the country.

In 1798, the celebrated traveller and circumnavigator, John Reinhold Foſter, publiſhed a geological tract which merits ſo much more attention, as all the facts were either obſerved by himſelf, or related to him by the immediate obſervers. In this he ſtates as a fact univerſally obſerved, that the ſouth and ſouth-eaſt ſides of almoſt every mountain are ſteep, but that the north and north-weſt ſides are gently covered and connected with ſecondary ſtrata in which organic remains abound, which he illuſtrates by various inſtances, ſome of which have been already, and others will preſently be mentioned.

J. R. Foſter; Univerſal fact: that the S. and S. E. ſides of mountains are ſteepeſt.

At preſent this fact attracts the greateſt attention, being obviouſly connected with the original ſtructure of the globe, and clearly proving that mountains are not mere fortuitous eruptions unconnected with tranſactions on the ſurface of the earth, as has of late been confidently advanced.

* It is to be regretted that he ſcarce ever quotes his authorities.

I shall now state the principal observations relative to this object, that have been made in different parts of the world.

In Europe.

Account of mountains: in Europe,

1. The mountains that separate Sweden from Norway extend from north to south, their western sides are *steep* and the eastern *gentle*, 1. Bergm. Erde Beschreib. p. 157.

2. The Carpathian mountains run from E. to W. their southern sides towards Hungary are steep, their northern towards Poland moderate, Foster, § 46.

3. Dr. Walker, professor of natural history at Edinburgh, observed that the coasts and hills of Scotland are steeper and higher on the western side than on the eastern; Jamison's Mineralogy of Shetland, p. 3. However, Jamison observed, that the south side of the isle of Arran is the lowest, and the north side the highest, p. 51.

4. The mountains of Wales are gentle on the eastern and steep on the western sides.

5. The mountains of Parthery, in the county of Mayo, are steep on the western side.

6. The mountains which separate Saxony from Bohemia descend gently on the Saxon or northern side, but are steep on the Bohemian or southern side; Charpent, p. 75. The southern declivity is to the northern as six to two, 2d Bergm. Journ. 1792, p. 384 and 385.

7. The mountains which separate Silesia from Bohemia run nearly from E. to W. yet are steeper on the northern or Silesian side than on the opposite Bohemian; Assemanni *Silesia*, 335. Such branches as run from N. E. to S. W. have their western covered with primordial strata, and consequently less steep; 4. New Roz. p. 157.

8. The Meissener in Hessia is steeper on the N. and E. sides which face the Warra, than on the south and western; 1. Bergm. Journ. 1789, p. 272.

9. The mountains of the Hartz and Habichtswald are steep on the south and gentle on the northern sides, Foster, § 46.

10. The Pyrenees, which run from E. to W. are steeper on the southern or Spanish side; Carbonieres, XIII.

11. The mountains of Crim Tartary are gentle on the northern and steep on the southern sides, Foster, ibid.

In

In Asia.

12. The Ourals, which ſtretch from N. to S. are far ſteeper on the weſtern than on the ſouthern ſides; Herman Geologie, p. 90, and 2d Ural Beſchreib. p. 389. In Aſia,

13. The mountain of Armenic to the weſt of the Ourals is ſteep on its E. and N. ſides, but gentle on the ſouthern and weſtern; 1. Pallas Voy. p. 277.

14. The Altaiſchan mountains are ſteep on their ſouthern and weſtern ſides, but gentle on the northern and eaſtern; Foſter, ibid. and Herman 2d. Ural Beſchreib. p. 390, in the note.

15. So alſo are the mountains of Caucaſus, 3d. Schrift. Berl. Geſelſch. 471.

16. The mountains of Kamſkatſka are ſteep on the eaſtern ſides, Pallas, 1 Act. Petropol. 1777, p. 43.

17. The Ghauts in the Indian peninſula are ſteep on the weſtern ſides.

18. The mountains of Syria which run from N. to S. ſkirting the Mediterranean, are ſaid to be ſteeper on the weſtern ſide facing the Mediterranean; 4. La Metherie, p. 380.

In America.

The Cordelieres run from N. to S. their weſtern flanks towards the Pacific are ſteep, their eaſtern deſcend gradually. In America;

In Guiana there is a chain of mountains that run from E. to W. their ſouthern flanks are ſteep, their northern gentle; Voyages de Condamine, p. 140.

To aſſign the cauſes of this almoſt univerſal allotment of unequal declivities to oppoſite points, and why the greateſt are directed to the weſt and ſouth, and the gentleſt on the contrary to the eaſt and north, it is neceſſary to conſider, Deduction of cauſes.

1. That all mountains were formed while covered with water.

2. That the earth was univerſally covered with water at two different æras, that of the creation, and that of the Noachian deluge. The earth was originally covered with water.

3. That in the firſt æra we muſt diſtinguiſh two different periods, that which preceded the appearance of dry land, and that which ſucceeded the creation of fiſh, but before the ſea had been reduced nearly to its preſent level; during the Two æras.

former

former, the primæval mountains were formed, and during the laſt, moſt of the ſecondary mountains and ſtrata were formed.

4. That all mountains extend either from E. to W. or from N. to S. or in ſome intermediate direction between theſe cardinal points, which need not be particularly mentioned here, as the ſame ſpecies of reaſoning muſt be applied to them, as to thoſe to whoſe aſpect they approach moſt.

In the firſt the primæval mountains were formed, and the waters moved from E. to W. and from N. to S.

Theſe preliminary circumſtances being noticed, we are next to obſerve that during the firſt æra, this vaſt maſs of water moved in two general directions, at right angles with each other, the one from E. to W. which needs not to be proved, being the courſe of tides which ſtill continue, but were in that ocean neceſſarily ſtronger and higher than at preſent: the other from N. to S. the water tending to thoſe vaſt abyſſes then formed in the vicinity of the ſouth pole, as ſhewn in my former eſſays. Before either motion could be propagated, a conſiderable time muſt have elapſed.

The primæval mountains reſiſted the waters, which occaſioned the greateſt depoſitions on the N. and E. ſides.

Now the primæval mountains formed at the commencement of the firſt æra, and before this double direction of the waters took place, muſt have oppoſed a conſiderable obſtacle to the motion of that fluid in the ſenſe that croſſed that of the direction of theſe mountains. Thus the mountains that ſtretch from N. to S. muſt have oppoſed the motion of the waters from E. to W. this oppoſition diminiſhing the motion of that fluid diſpoſed it to ſuffer the earthy particles with which in thoſe early periods it muſt have been impregnated, to cryſtallize or be depoſited on theſe eaſtern flanks, and particularly on thoſe of the higheſt mountains, for over the lower it could eaſily paſs; theſe depoſitions being inceſſantly repeated at heights gradually diminiſhing as the level of the waters gradually lowered, muſt have rendered the eaſtern declivities or deſcent, gentle, gradual, and moderate, while the weſtern ſides receiving no ſuch acceſſions from depoſitions, muſt have remained ſteep and craggy.

Again, the primæval mountains that run from E. to W. by oppoſing a ſimilar reſiſtance to the courſe of the waters from N. to S. muſt have occaſioned ſimilar depoſitions on the northern ſides of theſe mountains againſt which theſe waters impinged, and thus ſmoothed them.

Where

Where mountains interfect each other in an oblique direction, the N. E. fide of one range being contiguous to the S. W. flanks of another range, there the afflux of adventitious particles on the north-eaft fide of the one, muft have frequently extended to the S. W. fide of the other, particularly if that afflux were ftrong and copious; thus the *Erzgebirge* of Saxony, which run from W. to E. have their N. E. fides contiguous to the S. W. fide of the *Riefengebirge* that feparate Silefia from Bohemia, and hence thefe latter are covered with the fame beds of gneifs, &c. as the northern fides of the Saxon, and thereby are rendered fmooth and gentle comparatively to the oppofite fide, which being fheltered, remains fteep and abrupt, which explains the feventh obfervation. Mountains near each other would intercept this deposition.

The caufes here affigned explain why the covering of adventitious ftrata on the higheft mountains is generally thinneft at the greateft height, and thickeft towards the foot of the mountain, for the bulk of the water that contained the adventitious particles being proportioned to its depth, and the mafs of earthy particles with which it was charged, being proportioned to the bulk of water that contained them, it is plain, that as the height of water gradually decreafed, the depofitions from it on the higher parts of the mountains muft have been lefs copious than on the lower, where they muft have been oftener repeated. It would be leaft at the greateft heights, &c.

Hence, 2. granitic mountains, generally the moft ancient, frequently have their northern or eaftern fides covered with ftrata of gneifs or micaceous fhiftus, and this often with argillite, or primæval fand-ftone, or lime-ftone, thefe being either of fomewhat later formation or longer fufpendible in water.

Hence, 3. different fpecies of ftone are often found at different heights of the fame flank of a mountain, according as the water which conveyed thefe fpecies, happened to be differently impregnated at different heights; during the firft æra its depofitions formed the primitive ftony maffes, but after the creation of fifh, lime-ftone, fand-ftone, farcilites and fecondary argillites, in which pifcine remains are found, were depofited. But during the fecond æra, viz. that of the Noachian deluge, by reafon of the violence and irregularity of its aggreffion, the depofitions were more mifcellaneous and are found at the greateft heights; yet in general they may well

well be diſtinguiſhed by the remains of land animals, or of vegetables, or of both, which they preſent in their ſtrata (or at leaſt by the impreſſions of vegetables which they bear) as theſe muſt have been conveyed after the earth had been inhabited. But mountains regularly ſtratified bearing ſuch remains, for inſtance the carboniferous, cannot be deemed to have been formed in a period ſo tumultuous. During this deluge the waters alſo held a different courſe, proceeding at firſt from ſouth to north, and afterwards in both oppoſite directions, as ſhewn in treating of that cataſtrophe in my ſecond eſſay.

Exceptions from local cauſes.

Hence, and from various contingent local cauſes, as partial inundations, earthquakes, volcanos, the eroſion of rivers, the elapſion of ſtrata, diſintigration, the diſruption of the lofty mounds by which many lakes were anciently hemmed in, ſeveral changes were produced in particular countries that may at firſt ſight appear, though in reality they are not, exceptions to the operations of the general cauſes already ſtated.

Inſtances.

Thus the mountains of Kamſkatſka had their eaſtern flanks torn and rendered abrupt by the irruption of the general deluge, probably accompanied by earthquakes. And thus the Meiſſener had its E. and N. flanks undermined by the river Warra, as Werner has ſhewn; thus the eighth and ſixteenth obſervations are accounted for, as is the thirteenth, by the vaſt inundations ſo frequent in this country, 1 Pallas, p. 172, which undermined or corroded its E. ſide, while the weſtern were ſmoothed by the calcareous depoſitions from the numerous rivers in its vicinity.

Different ſpecies of ſtone muſt be found on different ſides of mountains.

Hence, 4. we ſee why on different ſides of lofty mountains different ſpecies of ſtones are found, as Pallas and Sauſſure have obſerved, 2 Sauſſ. § 981, a circumſtance which Sauſſure imagined almoſt inexplicable, but which Dolomieu has ſince happily explained, by ſhewing that the current which conveyed the calcareous ſubſtances to the northern, eaſtern, and north-eaſtern ſides of the Alps, for inſtance, was ſtopped by the height of theſe mountains, and thus prevented from conveying them to the ſouthern ſides, and thus the north-eaſtern ſides were rendered more gentle than the oppoſite, 3. New Rotz. p. 425. conformably to the theory here given.

Interceptions.

Hence, 5. where ſeveral lofty ridges run parallel to each other, it muſt frequently happen that the external ſhould intercept

tercept the depofitions that do not furmount them, and thus leave the internal ridges fteep on both fides.

Hence, 6. low granitic or other primitive hills are frequently uncovered by adventitious ftrata on all fides, as at Phanet in the county of Donegal, or are covered on all fides; the impregnated waters either eafily paffing over them, or ftagnating upon them, according to the greater or lefs rapidity of its courfe, and the obftacles it met with. **Low mountains.**

The twofold motion of the ancient ocean is noticed both by Buffon and Bergman, but neither of them have deduced from it the true explanation of the phenomena of which we here treat: Buffon attributes the formation of fecondary mountains to depofition or fediments from the fea after the exiftence of fifh, 1. Epoques, p. 143, in 8vo. which he fays invefted the bafes of mountains without noticing any diftinction of fides, p. 144 and 170. He thinks thefe fediments were equally conveyed from both poles towards the equator, for it is the equatorial regions that he thinks thofe mighty caverns opened, towards which the primitive ocean was impetuoufly borne and in which it was ingulphed, p. 181, 182, and 183. If fo, fimilar declivities fhould be formed on the fouthern as on the northern fides of mountains, which is contrary to the obferved facts. His explanation of the eaftern and weftern declivities is defective and erroneous, for he attributes the abruptnefs of the weftern fides to the erofion of the coafts on that fide (an erofion that exifts only in fancy) and the fmoothnefs of the eaftern to the gradual defertion and retreat of the fea on that fide, p. 184. and 185, a retreat equally fictitious, as De Luc has well fhewn. Whereas fince the general motion of the fea is from E. to W. if the erofion were of either fide it fhould rather be on the eaftern than on the weftern; befides, if the gentle declivities of the eaftern fides of mountains arofe from the gradual retreat of the fea, the petrifactions of the fecondary mountains thus formed fhould confift of fuch fhell-fifh as inhabit fhallow feas or fhores, whereas they confift chiefly of thofe called *pelagicæ*, which inhabit the greateft depths *. **Remarks on the ftatements of Buffon and Bergmann.**

With refpect to the eaftern and weftern declivities, Mr. Bergman's account of the origin of their inequality agrees exactly with mine, 2. Bergm. Erdeklotet § 183 and 187, but he

* 2. Bergm. Erdekugel, p. 315.

fails

fails in accounting for the inequality of the northern and fouthern, for he fuppofes the courfe of the water to tend equally from both poles towards the Æquator which would render the depofitions equal on both fides, which is contrary to obfervation.

XIV.

A Memoir on Animal Cotton, or the Infect Fly-Carrier. By M. BAUDRY DES LOZIERES, *Member of feveral Academies, and Founder of the Society of Sciences and Arts, at Cape Francois.* (American Tranf. V.)

GENTLEMEN,

Preface.

BEFORE I enter upon the fubject of this memoir, I ought to pay the tribute of praife which is due to due to your ufeful labours. But the ftyle of eulogy is ill fuited to the plainnefs of an American farmer, and while you are conftantly employed in *deferving* praife, you cannot fpare time to *hear* it.

Propofed new branch of Weft India commerce.

I am now going to communicate to you, with fome obfervations upon it, a fact of entomology which I have myfelf witneffed during my refidence at St. Domingo, and which, if I am not miftaken, deferves your greateft attention, becaufe it may introduce a new branch of commerce with the Weft India colonies, and render very ufeful an animal which has hitherto been known only by the mifchief which it occafions.

Every Inhabitant of the Weft Indies knows and dreads the greedy worm which devours their indigo and caffada plantations. But people have hitherto turned their attention more to the means of deftroying it than of rendering it ufeful. It is indeed very natural to endeavour to deftroy our enemy, but to compel him to be of fervice to us is by far the greater triumph.

Its Birth, Growth, and Death.

Production, growth, and death of the caffada worm.

The caffada worm is produced like the filk worm, that is to fay, from the eggs which the mother fcatters every where, after fhe has undergone her metamorphofis into a whitifh butterfly, or of a light pearl colour.

The

The egg is hatched about the latter end of July. Its developement is quick, for in September the worm is changed into a butterfly.

This month of September is the ſeaſon of his loves. The conſtant motion of his wings ſhews the ardency of his paſſion which he indulges day and night and even while feeding. The exceſs of this indulgence ſoon deſtroys him, he dies in the ſame month after violent convulſions.

I have ſaid that his life begins at the end of July. He is decked at his birth with a robe of the moſt brilliant variegated colours. This elegant livery, which nature ſeems to have delighted in forming, renders him always agreeable to the eye, which always dwells upon it with pleaſure.

Its Affinities.

It has appeared to me to be a ſmooth caterpillar whoſe external ſhape is exactly like that of the ſilk worm. External Appearances, &c.

It differs however from it, by its ſize, by its thickneſs, and by the beauty of its colours.

It again differs from the ſilk worm, becauſe it does not itſelf work the cone which I am going to ſpeak of.

I leave it to the learned to delineate its external configuration, and to determine upon the family of inſects to which it belongs. I ſhall only ſay that I do not believe it has, like the ſilk-worm, an inteſtine going in a direct line from the mouth to the anus, becauſe it appears to me that this cauſe of elaboration would not have the ſame deſtination.

Its Food.

It feeds on caſſada leaves, of which it is extremely greedy. Food. It feeds at all hours, day and night. It alſo nibbles the leaves of the potatoe, this is however but a tranſitory taſte, it ſoon returns to the caſſada leaf.

I have to obſerve that after it has taken its food, when the time of its metamorphoſis arrives, it does not purge itſelf by diet, like the ſilk worm, but continues to eat to the laſt moment.

The Approach of its Metamorphoſes.

In the month of Auguſt, and when on the point of undergoing its metamorphoſis, it ſtrips off its ſuperb robe, and puts Time and manner of its change.

on

on one of an admirable ſea-green, this fundamental colour reflects all its various ſhades, according to the different undulations of the animal, and the different accidents of light.

The Sting of the Ichneumon Fly.

Immediate depoſition of thouſands of eggs in its body.

This new decoration is the ſignal of its tortures. Immediately a ſwarm of ichneumon flies aſſail it. I think I am not miſtaken when I aſſert that there is not one of its pores that has not one of thoſe flies faſtened to it. There is even no neceſſity of making uſe of the microſcope to ſee that he is covered with them.

In vain he ſtruggles with all his might, raiſes himſelf upright to get rid of his cruel tormentors—He muſt ſubmit. Thoſe flies, of the ſmalleſt ſpecies, and which can only be ſtudied by means of the microſcope, drive their ſtings into the ſkin of their victim, over the whole extent of his back and ſides. Afterwards, and all at the ſame time, they ſlip their eggs into the bottom of the wounds which they have made.

After having performed this dreadful operation, the ichneumon flies diſappear, and the patient remains for an hour, in a drowzy and even motionleſs ſtate, out of which he awakens to feed with his former voracity. Then he appears much larger, and his ſize increaſes every day. His green colour aſſumes a deeper hue, and the tints produced by the reflection of the light are more ſtrongly marked. The animal in this ſtate of factitious pregnancy, if I may ſo expreſs myſelf, is worthy of all the attention of the obſerver of nature.

I ſhall not undertake the deſcription of the ichneumon fly, it is well deſcribed in the books. If I have obſerved a difference, it is the ſame which exiſts between the European *gnat* and the *muſquitoe* of hot regions, that is to ſay, that our Weſt-India flies are of a leſſer ſize.

I have now to deſcribe the operation which the ichneumon flies, which are extremely ſmall, perform at the very moment of their coming into the world; you will judge, gentlemen, whether this expreſſion is accurate.

Animal Cotton.

Fibrous productions called animal cotton.

A fortnight after the ichneumon flies have thus cruelly depoſited their eggs by perforating the unfortunate caſſada-worm, that

that is to ſay, ſome time in the month of Auguſt, thoſe eggs may be ſeen by the help of a microſcope, hatching on the body of that animal.

Thoſe eggs are all hatched at the ſame moment, and it is impoſſible to catch the moral point of time which may intervene between the birth of one and that of another. At one glance, the caſſada-worm is ſeen covered with all the little worms that have juſt been hatched. They iſſue out of him at every pore, and that *animated robe* covers him ſo entirely, that nothing can be perceived but the top of his head. He then turns to a dirty white, the little worms appear black to the eye, but their true colour is a deep brown. History of the inſects.

This operation laſts hardly more than an hour, and is followed by another which is not much larger but which is much more curious.

As ſoon as the worms are hatched, and without quitting the ſpot where the egg is which they have broke through, they yield a liquid gum, which by coming into contact with the air, becomes ſolid and ſlimy.

At the ſame time, and by a ſimultaneous motion, they raiſe themſelves on their lower extremity, ſhake their heads and one half of their bodies, and ſwing themſelves in every direction. Now is going to begin an operation which will afford the greateſt delight to the admirer of nature.

Each of thoſe *animalculæ* works himſelf a ſmall and almoſt imperceptible cocoon in the ſhape of an egg, in which he wraps himſelf up. Thus, they make, as it were, their winding ſheet. They ſeem to be born but to die.

Thoſe millions and millions of cocoons, all cloſe to each other, and the formation of which has not taken two hours, form a white robe in which the caſſada-worm appears elegantly clothed. While they are thus decking him, he remains in a ſtate of almoſt lethargic torpidity.

As ſoon as this covering is woven, and the little workmen who have made it have retired and hid themſelves in their cells, the worm endeavours to rid himſelf of thoſe barbarous gueſts, and of the robe which contains them, but he does not ſucceed in this attempt without the greateſt efforts.

He comes out of this kind of encloſure, entirely flaccid and dull, inſtead of his former fat and ſhining appearance, his ſkin now

now appears flabby, wrinkled and dirty, and gives him the appearance of decrepitude. He is now an exhausted, suffering being, threatened with approaching death.

He will still gnaw a few leaves, but he no longer eats with that voracious appetite, which indicated an active and vigorous constitution. Shortly afterwards he passes to the state of a chrysalis, and after giving life to thousands of eggs, he suddenly loses his own, leaving to the cultivator who has not yet bethought himself of calculating the advantage that he may draw from him, an advantage which may be so improved as to much more than compensate the ravages which he occasions.

Shell of the Ichneumon Fly.

Shell of the ichneumon fly.

I had imagined that the thousands of little worms which this shell contains in the cocoons of which it is composed, would be hatched some day. I shut it up therefore in a box closed with great caution. Every morning, and very often in the course of the day, I examined it, in order to catch the moment when those little animals were to be born a second time.

In fact, at the expiration of about eight days, I found the inside of the box lined with a cloud of little flies. I made myself certain that they issued out of the little cocoon. Several which issued out of them before my eyes, left me no doubt as to the fact.

I then took up some of those flies, and putting them on a pincer, I examined them with a microscope.

They are bold and lively: they have four wings. Their antennæ are long and vibrating, their belly hangs by a very fine thread: there are some that have a tail, and others that do not shew it. Afterwards I satisfied myself that they feed upon small insects that appear to be of the family of *Acarus*. Those indications appeared to me sufficient to be satisfied that they belong to the family of the ichneumon.

Observations on Animal Cotton.

The animal cotton is white and pure;

I have often held in my hand that cotton shell or wrapper. Its whiteness is dazzling. As soon as the flies have quitted the cocoon, it may be used without any preparatory precaution. It is made up of the purest and finest cotton.

I call

I call it *cotton* becaufe it is *idio-electric* and is pervious to the electric fluid. conducts electricity;

I add to this denomination the epithet *animal*, in contradiftinction to common cotton, which may henceforth be called *vegetable cotton*, fo that the two fpecies may be diftinguifhed from each other by their names, as they are by their origin, although they are very nearly related to each other in their effects.

It is to be obferved, that what might be called *cob-web* in the covering of the fly-carrier, or fmall flocks of filk which are probably intended to fhelter the animal from the rain, is far fuperior to what is called *ferrit* before, and *fleet filk* after the preparation of the finer filk. There is no refufe, no inferior quality in animal-cotton. Every thing in it is as fine and beautiful as can be imagined. the whole is ufeful.

It is poffible, if we may form a judgment by analogy, that medicine, which has extracted from filk what is called *English drops*, a remedy to which the greateft efficacy is attributed, may derive a fimilar advantage, perhaps for the cure of other diforders, from an extract of the animal cotton, which might be called the *St. Domingo drops*.

In fhort there is no need here of any of the precautions which the filk-worm requires. The robe which covers the fly-carrier, is worked every where, and every where perfectly well.

I fhall only obferve that as the rain fpeedily deftroys the caffada-worm, the inftant might be feized on when the ichneumon fly has depofited her eggs, to put the fly-carrier under fhelter. His natural food might be procured for him, as is done with the filk-worm. Caution againft rain.

The ichneumon fly never fails thus to come and depofite her eggs. I have never feen a fly-carrier that was not covered with the robe or fhell that I have fpoken of. I have continued this obfervation for many years, and the crop was fo abundant, that I alone, could collect in lefs than two hours, the quantity of one hundred pints, French meafure.

I repeat it, animal cotton is attended with none of the difficulties which occur in the preparation of vegetable cotton. It is fo pure, that as foon as the ichneumons have left it, which happens eight or ten days after their reclufion, it may be carded and fpun. Animal cotton judged to be much fuperior to the vegetable.

If

If it ſhould want any preparation, it could be only in caſe it ſhould not have been ſufficiently guarded againſt duſt and rain.

Vegetable cotton, beſides the ſeeds that produce it and with which it is charged, is filled with extraneous matter, of which it cannot be freed, but with a minute attention, many hands and much time, or with the help of machines which have not yet been brought to perfection.

In every point of view, animal cotton appears to me to have a great ſuperiority over that of the vegetable kind.

It will, perhaps, be wondered at, that experience has not long ago aſcertained this fact, but let it be conſidered that the ſilk-worm and its uſe, were known long before any uſe was made of them, and that we are now carefully repairing the loſſes that we have ſuffered by the careleſs indifference of our fore-fathers.

The fly-carrier may experience the ſame fate, becauſe it is leſs difficult to reaſon than to make experiments, but I dare hope that as ſoon as it ſhall have prevailed over the ſophiſtry of indolence, it will ſtand the competition with ſilk and vegetable cotton. It is more abundant than either. It requires leſs time and leſs trouble to procure it.

This cotton makes good lint.

I have but one word more to add. Silk and vegetable cotton ſerve only to envenom and inflame wounds, which is attributed to the aſperities of their filaments; I have frequently employed animal cotton as lint in the hoſpital of my plantation, it has always ſupplied the want of that made of flaxen linen, and I have not obſerved the ſmalleſt inconvenience to ariſe from the uſe that I have made of it.

Had it not been for the troubles that have laid our colony waſte, and which have prevented the neceſſary communication, I ſhould have brought to you a fly-carrier in every one of the periods of his life. You would have ſeen the eggs, the magnificent robe with which he is decked at his birth, the kind of food that he is fond of, the ſimple but noble veſtment in which he wraps himſelf up on the approach of his tormentors, you would have ſeen thoſe covering his whole body as it were with points, you would have ſeen him covered with his ſhell, and that ſame ſhell carded, ſpun and ready for the weaver. I had in a great degree already executed this deſign.

But

But it is too well known that I have not been able to save any thing in my flight from home, you will, however, be able at a future day to ascertain the truth of the fact that I have stated to you. I thought that a fact of this nature deserved to be deposited among your archives, and I may perhaps request of you the permission of depositing there some other still more curious facts.

By. DES LOZIERES.

Philadelphia, Feb. 3, 1797.

XV.

An Essay on the Fecula of Green Plants. By Professor PROUST.

HILAIRE ROUELLE is the first who discovered a substance in the green fecula analogous to the gluten of flour. This substance, since that time, has never appeared doubtful, because there are few chemists who have not seen its true characters. The fecula, of which it is the basis, in the opinion of Fourcroy, is only an imaginary being, or, at most, it has been too slightly examined to be admitted in the number of immediate vegetable products; he even proceeds so far as to suppose that the albumen, that animal product, which no one ever before suspected to be contained in plants, is the substance which ought to be admitted instead of the glutinous part of green feculæ.

Opinion of Fourcroy that the fecula of plants contain albumen and not gluten, as Rouelle asserted.

Are albumen and gluten found together or separate in the juice of plants? This is the question I have proposed to solve, and I shall endeavour to resolve it in this part of my observations on his "System of Chemical Knowledge."

are both found in plants?

To save my readers the trouble of turning to the work, I shall copy the passage in which the author has collected the facts and arguments upon which he has formed his judgment. This passage is still more remarkable for the difference it presents between his manner of characterising other vegetable products, and that of the chemists of the present day.

"Rouelle the younger, who examined and particularly compared it with animal matters, asserts that he has found it in the coloured feculæ, and especially in that which is called the green fecula of plants. But the name of fecula being

Quotation from Fourcroy.

given

given indifferently to the fibrous matter contained in the juice of plants, and to the ſtarch, has induced chemiſts to conſider the latter as part of the reſidue of ſolid vegetable ſubſtances, and there is reaſon to think that it was only by analogy, or perhaps by certain equivocal properties, that Rouelle believed the green matter contained any of the glutinous ſubſtances; ſubſequent experiments at leaſt, and ſuch as I have many times repeated on coloured feculæ, have not yielded me the confirmation of this aſſertion, nor has any thing been aſcertained with certainty, that the gluten is one of the principles of this latter fecula."

The name of fecula, ſays Fourcroy, being given *indifferently* to the fibrous matter contained in the juice of plants, and to the ſtarch, has induced chemiſts to conſider the latter as part of the reſidue of ſolid vegetable ſubſtances, and there is reaſon, &c.

He accuſes the ancient chemiſts of inaccuracy,

I ſhall firſt notice that this opinion is not correct. For example, the chemiſts of the preſent day will never agree with Fourcroy, that the confuſion ariſing from the improper application of terms, of which later chemiſts have juſtly complained, has, by a neceſſary conſequence, produced inaccuracy in thoſe who have preceded us. Our maſters, it is true, gave bad names to things, but they did not confound them more than we do.

which is not well founded.

Even at the time when every vegetable precipitate was called a fecula, the ſimilarity of terms never miſled them ſo far as to cauſe them to confound the ſtarch with the reſidue of the ſolid parts of plants. Firſt, we are not acquainted with any reſidue of this deſcription, to which chemiſts can reaſonably compare it; and ſecondly, if any of them did take the green fecula for a reſidue, there was not one of them who did not perfectly know the difference between this fecula or reſidue and ſtarch; and as no ſuch confuſion is to be found in their works, the reproach is unjuſt; we need only look into thoſe of Rouelle, Macquer, Baumé, Sage, Parmentier, &c. to be convinced that the term of fecula has never miſled theſe authors into aſſimilations ſo diſcreditable to their judgment, as to place in the ſame rank the green fecula, the reſidue of the ſolid parts, and the ſtarch.

The fecula was never confounded with fibrous matter.

We will now proceed to the green feculæ, and affirm, that in the laboratories in pharmacy, and ſtill leſs in the hands of a chemiſt ſo celebrated for accuracy of obſervation as Rouelle, the

the broken fibres of green plants have never been confounded with that beautiful soft liquid expressed from their leaves, or with that emulsive product which passes pure through the strainer, and which, by its excessive fineness and the brilliancy of its colour, differs so very much from the herbaceous filaments.

Again, if it were true that the fecula were a body homogeneous with the rest of the plant; if it were possible to consider it as in no respect different from the other parts, but by being more bruised, would it not be possible, by compleating the trituration of the remainder, to convert it also into fecula? When a fresh plant is bruised by the pestle, it is broken small; its texture is destroyed, but it is not pulverized. **Its state of diffusion in water is not from grinding.**

This crushing for a few moments, differs too much from a dry pulverization, to admit of any comparison between the fecula so produced, and a moistened powder. When an aqueous juicy plant, a sedum for example, is crushed with a roller on the slab, its expressed juice will afford fecula. Certainly it is not to the trituration, that a fecula is indebted for its softness, its fineness, and the impalpability which distinguishes it from powders. It is molecular in its own nature, and is even, perhaps, crystallized in those fibrous cavities where it is deposited by vegetation.

Rouelle asserts, according to Fourcroy, that the feculæ contains a principle similar to animal matters, &c. Rouelle does more than that: little contented with simple assertions, he proves it, not by *analogy* or *equivocal properties*, but by a succession of convincing facts, by approximations which have been universally admitted, because they combine together the most prominent characters which were then known, or are even yet known, to exist in animal substances. Whence otherwise could Rouelle have drawn his analogies, to enable him to compare, as he does, the green fecula with the gluten of wheat? In fact, what is there in the common appearance of these two products that can lead to the comparison? Their points of comparison must be sought for in their composition, in their chemical properties, and this was done by that indefatigable chemist. These are approximations drawn from analysis which serve as the basis of the Memoir he has written on the green feculæ, and of which there is no mention in the System of Chemical Knowledge; doubtless because in the opinion **The doctrines of Rouelle were not mere assertions, but clear investigations from numerous facts.** **which Fourcroy has not detailed.**

opinion of its illustrious author, Rouelle had confounded albumen with the gluten, and the detail of his supposed mistake was considered as a matter of indifference in the history of chemistry.

Rouelle first discovered the peculiar animalized substance of the green fecula,

Rouelle however found in the fecula of sorrel, a product so amply possessing the chemical properties of albumen, that he particularly insisted upon it in order to fix the attention of his time upon a substance so animalized; and as he afterwards obtained it from a plant, which, according to Fourcroy, does not yield the slightest trace of albumen, it is now incontestable, as it was then, that Rouelle was the first who discovered in the juices and green feculæ a *product* which, if not intitled to the name of albumen, possesses, nevertheless so strongly, all the properties by which the attention of chemists has been called exclusively to it, that it is no less proper to be urged in the history of their discoveries, than albumen itself.

and the identity of veg. gluten and cheese.

It is to the same penetrating eye, the same impulse of genius which led him to anticipate discovery, that he is indebted for that of the astonishing resemblance between caseum and gluten, when they have both undergone that species of fermentation which transforms them into the cellular combination, the odorous and savory compound, called cheese. And the gluten, in this singular result, resembles the other more completely the more carefully it has been washed. Macquer, when he published what continues to be every where repeated, that part of these changes were occasioned by some remains of starch, was incorrect in his notion. Starch, a substance always inactive in fermentation, in that of bread, of beer, and even in germination, could only retard the effect produced by the gluten itself, and consequently could only destroy, in part, those characters, from which Rouelle stated the resemblance of these two products.

Peculiar fermentation of cheese.

And even their analysis extends far beyond the limits assigned to them; for when the gluten has changed its insipid and viscous mucosity for the cheesy state; when it has gone through all the stages of that fermentation which is essential to that condition, it is found also to have acquired the taste of those sharp and burning salts which constitute the principal merit of the Roquefort cheese; salts which have nothing in common with what is added, but are found equally powerful in the curd which has been washed and left to its own fermentation.

In fact in the cheese from gluten, as well as in that from animals, potash and sulphuric acid enable us to find that ammonia, and that vinegar discovered by Vauquelin. Is the ammoniacal acetite then one of the ingredients which gives the flavour to cheese? I only know that alcohol applied to strong cheese deprives it of all its taste. An analysis of that species might afford curious results; but let us return to the green feculæ; examine them by the lights of modern chemistry, and endeavour more particularly to discover whether albumen actually exists where Beccari and Rouelle found gluten. Salts generated in cheese, &c.

Green Fecula.

I. The fecula undergoes, by heat, a change capable alone of giving it a decisive character as to its nature. I mean that concrescibility which has so few examples in vegetable products; that agglutination which compresses the particles together, and produces the appearance of a cheesy curd. Though the fecula, before this change, passed easily through the strainer, it can no longer do so after having been heated; a peculiar hardening has deprived it of its tenuity; but heat does not coagulate the fibrous tissue; in this respect the fecula does not resemble the broken straw of green plants. The green fecula very concrescible by heat.

II. The fecula, separated from the juice by filtration, acquires an elastic and horny consistence by drying. It is softened with difficulty in heated water, but will not become soft even at the end of a month; notwithstanding it is moistened it still retains its horny state. When bended it will return to its shape, and cannot by any means be crumbled: these qualities also are not found in the dry woody pulp. Elastic and horny when dry,

The feculæ of green and white cabbages, cresses, hemlock, &c. do not lose the property of coagulating by heat from that cause. Into water heated to between 50 and 60 degrees, plunge two matrasses of equal size, one containing diluted fecula, and the other with the water and white of an egg; the fecula will harden and be collected in flocks, such as are formed in a juice which is clarified by heat; but at this temperature the albumen will not even lose its transparency. More concrescible than white of egg;

III. The green fecula is very nearly in equipoise with water, for that of plants which are not acid, frequently requires a week to precipitate.

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precipitable from water by alcohol and by acids.

Put some fecula washed and diluted into three glasses of equal size. To the first add a little alcohol, a few drops of acid to the second, and place the third between them for comparison. In the two first the precipitation will be compleat in half an hour, while in the third it will be hardly begun; alcohol and the acids therefore coagulate feculæ; but they have no such action upon the woody residue.

Alcohol takes up near one sixth of resinous matter.

IV. One hundred parts of the dried fecula of hemlock yielded to alcohol from 15 to 16 of green resin. After repeated infusions to which it was subjected, it remained of an earthy grey, and the alcohol was incapable of bleaching it. Sage, who was well acquainted with feculæ, found that they yielded about one third of their weight in resin; in order to separate it easily, the fecula must be thrown into spirits of wine while yet moist, the fluid then penetrates and attacks all its parts; but the effect is much more difficult when it has become horny by drying.

(To be continued.)

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Prize Questions of Foreign Learned Societies.

National Institute. Prize questions.

THE National Institute of France held a public sitting on the 20th Vendemiaire, when the new subjects for prizes were announced.

The *Class of Mathematical and Physical Knowledge* proposed the following question:

Winter sleep of animals.

"To determine, by anatomical and chemical observations and experiments, what are the phenomena of inactivity which certain animals, such as marmots, dormice, &c. undergo in the winter, with regard to the circulation of the blood, the respiration and the irritability; to inquire what are the causes of their sleeping, and why it is peculiar to these animals."

This question was proposed before, and the prize was to have been decreed in this sitting; but the meeting being of opinion that the memoirs received did not contain sufficient information, decreed that it should be proposed again, and that the prize should be doubled. It will be of the value of two kilogrammes of gold (about 6.800 fr.); and will be distributed

tributed at the public fitting of Vendemiaire in the year 13. Memoirs muft be tranfmitted to the Secretary of the Inftitute before the 15th Meffidor of the year 12.

The queftion propofed by the *Clafs of Moral and Political* knowledge is,

" To determine how the faculty of thought can be decompofed; and what are the elementary faculties which can be difcovered in it?" Decompofition of thought.

The prize is a gold metal of the weight of five hectogrammes (about 1700 fr.): it will be adjudged at the public fitting of Germinal in the 12th year of the Republic.

The works cannot be received after the 15th Nivofe of the fame year.

Prize for Geography. " To compare the geographical charts by Ptolemy, of the interior of Africa, with thofe which have been tranfmitted to us by fubfequent geographers and hiftorians, with the exception of Egypt and the coafts of Barbary from Tunis to Morocco." Geography prize.

This fubject had been propofed in the year 9, but the memoirs fent not correfponding to the conditions of the notice, it was renewed.

The prize is a gold medal weighing five hectogrammes (about 1700 fr.), and will be adjudged in the public fitting of Meffidor in the year 12. The memoirs muft be fent in before the 15th Germinal of the fame year.

The Medical Society at Montpellier has propofed two prizes of 500 francs each, of which the firft is to be adjudged in May 1803, and the fecond in May 1804, on the following queftions: Med. Soc. of Montpellier. Prize queftions.

1ft. In what kinds of difeafes, and under what circumftances, is inflammation favourable or dangerous? and, in the treatment of fuch difeafes, in what cafes ought it to be excited or checked. Inflammation?

2d. To afcertain by experiment and obfervation, what degree of confidence can be placed on the ufe of certain fubftances, by friction, which are generally adminiftered internally; to determine the effect of fuch remedies, in both methods, and alfo the quantity of the dofe; to point out the difeafes and cafes in which one method is to be preferred to the other; and finally, to determine, in the different maladies, to what parts of the body the application of thefe remedies, are moft efficacious. Medicines by friction?

Flafh

Flash from an Air-Gun.

Luminous flash from an air gun.

M. Pictet, in a letter from Paris of the 1st of January last to Mr. Tilloch (Philos. Magazine, Vol. XIV. 363.) states a communication to the National Institute of France on the 29th of December by M. Mollet, of Lyons, respecting the luminous appearance produced by the discharge of an air-gun in the dark, a phenomenon which he considers as never having been before observed. It has, however, been known for some time in this country, having been first mentioned, I believe, near a year and a half ago by Mr. Fletcher at a meeting for philosophical experiments and conversations, which was then held weekly at my house. Several discussions accordingly took place with regard to its cause,—as, whether it was produced by electricity, or the change of capacity in the expanding fluid, &c. and it was intended to institute a course of experiments on the subject, but some other objects of enquiry intervening the matter was postponed. It is a curious phenomenon, and deserves investigation.

Letter from Professor PROUST *to J. C.* DELAMETHERIE.

On the Sugar of Grapes, and the Compound Nature of Urée.

New sugar in the grape.

I have discovered a new sugar in the grape, which is the basis of wine; it is different from that of the sugar-cane, crystallises differently, &c. It is contained in the proportion of about 30 per cent. in the juice of the grape. Azote is uniformly combined with the carbonic acid in the fermentation of wine: in that of gluten it is pure hidrogen, which is disengaged with the carbonic acid.

The urée is a compound containing ammonia.

Tell Vauquelin that the urée, in the state in which they have examined it, is a saline substance saturated with ammonia, and not a simple product; he need only apply sulphuric acid to carry off this ammonia, and to have the urée pure, but coloured with a resin from which I have not yet been able to free it.

J. de Physique.

On

On the Use of Sulphate of Soda in the Manufactory of Glass. *By* PAJET-DESCHARMES *.

Since the notice which was given in the *Journal de Physique* that fulphate of foda might be ufed, without preparation or any intermediate agent, in the fabrication of filiceous glafs, Cit. Pajot-Defcharmes has thought fit to publifh the refult of his principal experiments on the employment of this falt in glafs-making. Ufe of fulphate of foda in glafs making.

It appears from the experiments which this operator has reported in the *Journal de Phyfique,*

1. That fulphate of foda and fand alone, in various proportions cannot fucceed.

2. That fulphate of foda, mixed with pounded charcoal, in the proportion of a tenth or twentieth part, yielded a yellow glafs more or lefs black, of the nature of obfidian paftes or ftones: the crucibles were then very flightly acted upon.

3. That equal parts of carbonate of lime, dried fulphate of foda, and fand, produced a beautiful glafs, clear, folid, and of a pale yellow: the crucibles were then very little corroded.

Cit. Pajot-Defcharmes obferves, that notwithftanding the pains he took, he could never obtain a glafs with fulphate of foda that was not of a yellowifh green, whereas muriate of foda (fea-falt), treated in the fame manner as fulphate of foda, always produced a glafs of a light blue tinge inclining more or lefs to green.

Cit. Pajot-Defcharmes propofes to treat more fully, in a particular memoir, on all the experiments he has made on this fubject.

Obfervations on the Neceffity of immerfing Seeds in Water in Times of Drought. *By Ant.* ALEXIS CADET-DE-VAUX. †

Franconville-la-Garenne, 2 Brumaire (Oct. 23.)

To reap, we muft fow; and, from the drought which has prevailed for fix months, fowing is not eafy, for our gardens In dry feafons the feed grain is in danger of perifhing in the ground.

* Abridged with remarks in the Journal des Mines, No. 69. from a Letter from Cit. Pajot-Defcharmes, to J. C. de la Metherie, in the Journ. de Phyfique, Vol. LII, p. 210.

† Decade Philofophique, No. 4. An. XI.

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are no longer cultivated with the ſpade and the harrow, but with the mattock and the pick-axe; the plough, however, on account of its ſtrength, can ſtill be employed to till the ground, except in compact lands, and thoſe which are ſtiffly bound. But it is not enough that the land ſhall have been tilled to enable us to ſow it, the ſeed muſt alſo germinate, without which it dries and periſhes, or becomes the food of animals and inſects. For without rain, or dew which moiſtens at leaſt the ſurface of the ſoil, there can be no germination. In the mean time the ſeaſon advances, and the ſeed-time is already late. Let us point out, then, to the huſbandman, a method of preventing the inconvenience of drought; it is, not to commit his ſeed to the earth, until it is impregnated with the moiſture neceſſary for its germination.

Remedy; to ſoak it firſt in water.

We may refer to a great example: the Chineſe do not depoſit a ſingle ſeed in the earth until it has been immerſed in water. And I will adduce an experiment nearer home, which is in favour of this practice. Five years ago, I ſowed half an acre (*arpent*) of land with wild ſuccory, lucern, and pimpernel. With a view to compare the produce of theſe plants, I ſowed them in rows two feet aſunder. The ſpring was very dry, and I ſoaked each of the ſeeds in water for forty eight hours. The quantity ſoaked was not ſufficient; for there was required as much more as to ſow nine rows, three of each kind. I took advantage of this circumſtance to compare the effect ariſing from the ſeed being prepared or not prepared by immerſion. The reſult was, that in the nine rows ſowed with the dry ſeed, not more than thirty plants came up in five months: while the remainder of the land was covered, and formed a moſt beautiful artificial meadow. The rain fell too late to ſow theſe nine rows, and it was neceſſary to ſow them again in autumn.

Experimental proof.

Let us now apply theſe facts to the ſowing of corn.

Liming recommended; by immerſion.

The huſbandman is in the habit of liming his wheat when he apprehends the rot. This year he has no need to dread that evil; nevertheleſs let him uſe the lime, but by immerſion; for in general the method of application is defective. The operation is uſually confined to a ſimple ſprinkling of the heap of wheat with lime water, while it is turned over with a ſhovel.

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The good, the only way of liming, is by immerſion: put the ſeed into tubs, and cover it to the height of four or five fingers breadths with lime-water, made ſo hot that the hand cannot be kept in it without difficulty; cover it up, ſtirring it three or four times in the twenty-four hours; after which draw out the bung that the water may run off, the quantity will be but ſmall; it will be nearly all abſorbed by the grain, which muſt be taken from the tubs, ſpread out in the air and then ſowed. Method deſcribed.

Twelve buſhels of wheat, immerſed for twenty-four hours, will abſorb nearly one-fourth of water, that is to ſay, they will ſwell to fifteen or fixteen buſhels by meaſure. Let us now inveſtigate the theory. Every grain of this corn carries with it to the earth a quantity of water, more than ſufficient to enſure its germination. This water acts principally upon the extractive matter of the huſk; it diſſolves this principle, one of the properties of which is to attract and ſtrongly to retain moiſture. Hence this water will not be evaporated. If inſtead of pure water, we uſe dunghill water, which is ſaturated with this extractive matter, together with deliqueſcent ſalts, and fatty matter, then the moſt minute quantities of ſurrounding humidity will be attracted towards the grain. But in truth it will, after this treatment, ſucceed very well without the ſpeedy aſſiſtance of the rains and dews; it poſſeſſes a ſufficiency of moiſture to put forth its germ, to throw out its radical, and in ſhort, to ſecure its germination. The grain which has been ſteeped gains, in ordinary ſeaſons, from twelve to ſixteen days in advance before that which has not been ſteeped; and in times of exceſſive drought, it gains every thing. If ſteeped, it germinates and grows; and if not ſteeped it dries and periſhes. Let the rains come, let them continue, ſtill I adviſe immerſion; which by forwarding the germination, remedies the inconveniencies of a late ſeed-time.

Wheat becomes one fourth larger by abſorbing water.

Dunghill water is beſt.

Recapitulation.

Extract from a Memoir by CIT. FOURCROY, *on the Chemical Nature of Ants, and on the ſimultaneous Exiſtence of two Vegetable Acids in theſe Inſects.*

Samuel Fiſcher firſt deſcribed this acid in 1670. It has ſince been more particularly examined by Margraff, Arvidſon, Bergman, &c. and finally by Cit. Deyeux, who eſtabliſhed the identity, which Margraff had previouſly ſuſpected to

Experiments ſhewing the exiſtence of malic as well as formic acid in Ants.

Experiments ſhewing the exiſtence of malic as well as formic acid in ants.

to exiſt between the formic and acetous acids. Nevertheleſs there were ſtill ſome doubts to remove, and it was theſe which induced the Citizens Fourcroy and Vauquelin to undertake the following inveſtigations:

Some red ants (*formica rufa,* Lin.) were cruſhed in a marble mortar. A ſharp vapour was diſengaged, ſimilar to that of radical vinegar; and the alcohol, in which the ants were put to macerate, was tinged yellow.

This infuſion produced, by diſtillation, an inflammable liquor, ſlightly acid. At the ſame time it formed a browniſh ſediment, which was carefully ſeparated. This ſediment became covered with an acid liquor, which was ſaturated with lime.

The latter combination became brown and thick: it had a pungent nauſeous taſte, and the air produced bubbles in it, as in ſoap-ſuds.

One part of this compound, mixed with one part and a half of ſulphuric acid and two of water, produced a very thick magma, which, by diſtillation, yielded an acid liquor, without colour, of an empyreumatic odour, but which did not diſcover any trace of ſulphuric acid.

This acid combined with potaſh, formed a true acetite.

The browniſh thick compound, of which we have ſpoken above, formed, by ſolution in acetite of lead, an abundant precipitate, which proves that the acid extracted from the ants by the alcohol, contained ſomething beſide the acetous acid.

The ſame calcareous compound mixed with a ſolution of nitrate of lead, yielded an abundant yellow precipitate, which, treated with ſulphuric acid weakened by water, formed a new precipitate, heavier and whiter. The ſupernatant fluid was ſlightly acid and ſugary: it precipitated abundantly the nitrates of mercury, of ſilver, and of lead.

Many other facts, in addition to thoſe we have mentioned, are ſufficient proofs that the malic acid is joined with acetic acid in the liquor which is extracted from the ants by the alcohol; and it is doubtleſs the preſence of this acid which has led thoſe chemiſts into an error, who formerly treated of this ſubject.

The ants, after having been ſeparated from the alcohol, yielded, by diſtillation, an empyreumatic fetid oil, carbonate of

of ammonia and acetite of ammonia, the whole diffolved in a great quantity of water.

The brown fubftance left from the diftillation of the infufion in alcohol, was infoluble in water, and foluble in alcohol, except a fmall quantity of a brownifh matter, which appeared to the authors to be albumen. The folution of this brown fubftance in alcohol became milky by the addition of water; and, in a few days, depofited a precipitate of a refinous appearance, which feemed to be a fat matter of a peculiar nature.

Laftly, the refiduum from the ants was an animal carbon, which, after combuftion, left only phofphate of lime.

The memoir finifhes with obfervations on the prefence of the acetous and malic acids in ants in particular, and in organic bodies in general.

Bulletin des Sciences, No. 70.

An Effay on the Relation between the fpecific Gravities and the Strengths and Values of Spirituous Liquors; with Rules for the Adaptation of Mr. Gilpin's Tables to the prefent Standard, and two New Tables for finding the Per-centage and Concentration when the fpecific Gravity and Temperature are given. By ATKINS and Co. *Mathematical Inftrument Makers. Quarto,* 74 *Pages. London, Cadell and Davies, and Robinfons,* 1803.

Atkins on the fpecific gravities of fpirituous liquors.

It is certainly very remarkable, that no mode of denomination by which the real ftrength of fpirituous liquors could be defined with any tolerable degree of correctnefs, has ever yet been in general ufe in this or any other country. The mifchiefs accruing from the want of fuch a fyftem, and the difputes which were daily arifing between the revenue officers and the traders, with refpect to thefe matters, induced the legiflature feveral years fince to pafs an act by which was declared, that all fpirits fhould be deemed and taken to be of the degree of ftrength which fhould be denoted by Clarke's hydrometer. The inconveniences, however, which refulted both from the complex conftruction of the inftrument itfelf, and the ineligibility of the mode by which the ftrengths of thefe liquors are denominated, according to the fyftem propofed by its inventor, having been long felt, and both one and

Atkins on the ſpecific gravities of ſpirituous liquors.

and the other having accordingly in a great meaſure fallen into diſuſe amongſt all but thoſe immediately concerned in the collection of the duties, the lords of the treaſury during the laſt ſeſſion of parliament, applied for and obtained an act, empowering them to order any other hydrometer to be uſed for the purpoſes of the revenue, which might be found more conveniently applicable to them. This has of courſe rendered a review of the whole matter indiſpenſably neceſſary, and the authors of the pamphlet before us, whoſe attention to this ſubject is already ſufficiently known, have accordingly with great perſpicuity traced the principles on which alo an equitable ſyſtem can be eſtabliſhed.

After ſhewing the conn tion between the ſubject of the preſent tract, and the apprec tion of our weights and meaſures in general, they proceed in the preface to ſtate the authorities on which they have f ded their eſtimation of the weights of the known m ſures of diſtilled water. Theſe are principally the experiments of Sir George Shuckburgh Evelyn *, and thoſe of the French commiſſioners of weights and meaſures. From the former it appears t reſult, that the cubic inch of diſtilled water at 60° of Fahrenheit's thermometer, weighed in air at the ſame temperature when the barometer ſtands at 29½ inches, is equal to 252.506 troy grains, and from the latter, equal to 252.55, ſo that the authors take it to be under the circumſtances ſufficiently near 252½, and that this weight therefore correſponds vith the ſpecific gravity of 1,000. Their appreciation of the cubic meaſure of the wine gallon is ſimilar to that of e board of exciſe, viz. that it contains 231. cubic inches, and they eſtimate the pound avoirdupoiſe at 7,000 grains troy. The preliminaries and a detail of the calculations and reaſons on which the deductions are founded, are very properly introduced in this place, as neceſſary to the underſtanding of their ſubſequent eſtimation of the ſpecific gravity of proof ſpirit.

The work itſelf treats of the ſubject under the following general heads; of the general relation between the ſpecific gravities and the ſtrengths and values of ſpirituous liquors, and the circumſtances by which the former are influenced; of the

* See Phil. Journal, Quarto Series, Vol. iii. p. 97, &c. and Octavo Series, Vol. iv. p. 35. (No. for January laſt.)

 ſtandard

ftandard of proof; of over-proofs and under-proofs, and the modes of appreciating them; of Mr. Gilpin's experiments and tables; and of the means of adapting them to the prefent ftandard, with various problems and rules for that purpofe; and concludes with the two tables of the authors mentioned in the title-page.

Atkins on the fpecific gravities of fpirituous liquors.

The firft chapter treats very perfpicuoufly of the fubject in general, and the intricacy in which it is involved by the joint effects of concentration and change of temperature.

In the fecond the authors proceed to the eftimation of the ftrength of proof fpirit, as deducible from a loofe paffage in an act of parliament paffed in 1762, by which it is enacted, that "for the purpofes of that act, each gallon of brandy or fpirits of the ftrength of one to fix under hydrometer-proof, fhall be taken and reckoned at 7 lbs. 13 oz. the gallon; and which feems to be the only claufe in the ftatute-book, in which the ftrength of any kind of fpirituous liquor is attempted to be defined by reference to its fpecific gravity. The temperature of the liquor and fome other circumftances are here left to be affumed, which the authors have accordingly been under the neceffity of doing, and from hence they deduce, that proof fpirit may reafonably be confidered as having the fpecific gravity of 920 at 60°. according to the common acceptation; and this they accordingly recommend as the future legal definition of that ftandard.

The third chapter contains a diftinct account of the various modes of comparifon hitherto in ufe, and of their refpective inconveniences and defects. According to the fyftem propofed by the authors, the denomination itfelf would at once indicate the real comparative value of the fpirit in queftion, by reference to the equivalent quantity of proof fpirit, or that which would produce or be producible from 100 gallons of the former, by the addition of water to the ftronger of the two, till they were reduced to the fame degree of ftrength; allowance being made in all cafes for the concentration which takes place by mixture, and the change in bulk and confequent difference in value by meafure, according to the exifting temperature. Thus, an under-proof of which 100 gallons might be made up with water from 80 gallons of proof, would be called fpirit of "80" or "20 under-proof," and a fpirit of which 100 gallons would when reduced to proof, make 134, would be called "134,

or

Atkins on the specific gravities of spirituous liquors.

or "34 over-proof." We here find fome arithmetical rules introduced for the purpofe of illuftrating the practical facilities refulting from the ufe of fuch a fyftem, and the chapter concludes with a fection on the general conftruction of the hydrometer, in which, however, the authors have modeftly forborne to fpeak particularly of thofe of their own conftruction, which are already fo well known to the public.

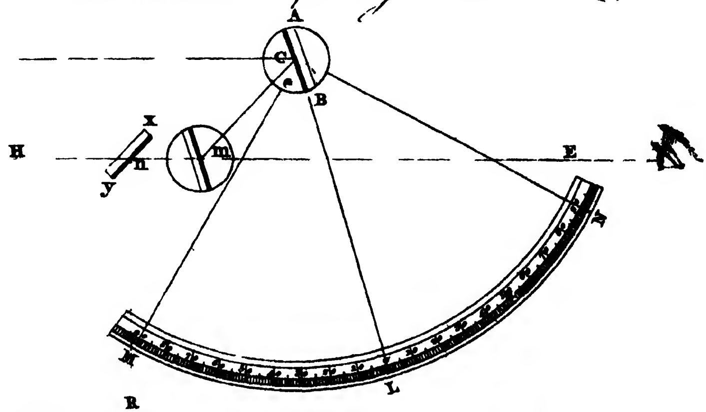
In the fourth chapter, which treats of the experiments and tables of Mr. Gilpin, the author fpeaks with its merited praife of this great work, and in the fifth they give a variety of rules, fome of them very curious, for folving all the moft ufeful problems refpecting this fubject from thefe tables, which they confider as an authentic regifter of experimental refults. Thefe proceffes are all founded on the fuppofition that proof fpirit is of the fpecific gravity of 920 at 60°.

The fixth and laft chapter contains the two tables of the authors already mentioned, with their ufe and application. Thefe tables in a great meafure anfwer all the purpofes of the more voluminous ones of Mr. Gilpin, from which they are calculated by the preceding formulæ, and that in a much fimpler and eafier manner, being adapted to the prefent ftandard of proof, and giving the required per-centage and concentration by mere infpection.

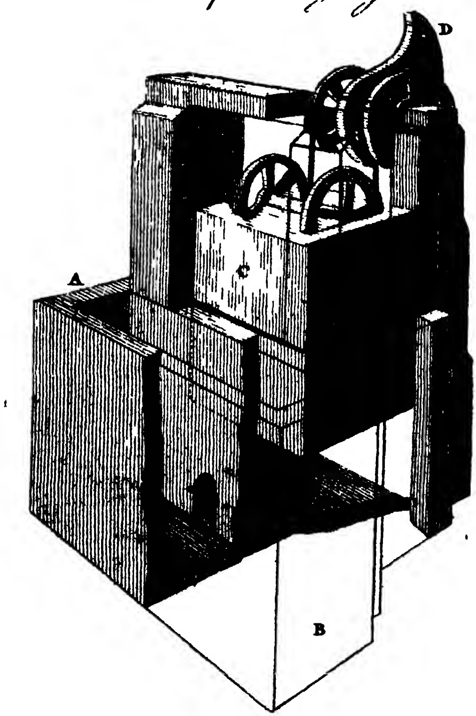
Having thus ftated the outlines of this work, it would be to take notice of a circumftance of minor confequence if I remarked, that it is a very neat fpecimen of typographical elegance. The fubject is a moft important one, and the philofophical reader will be pleafed to find it here treated in a manner which renders it no lefs worthy of the attention of the man of fcience, than of thofe whofe commercial interefts are involved in the event of the prefent inveftigation.

A. B.

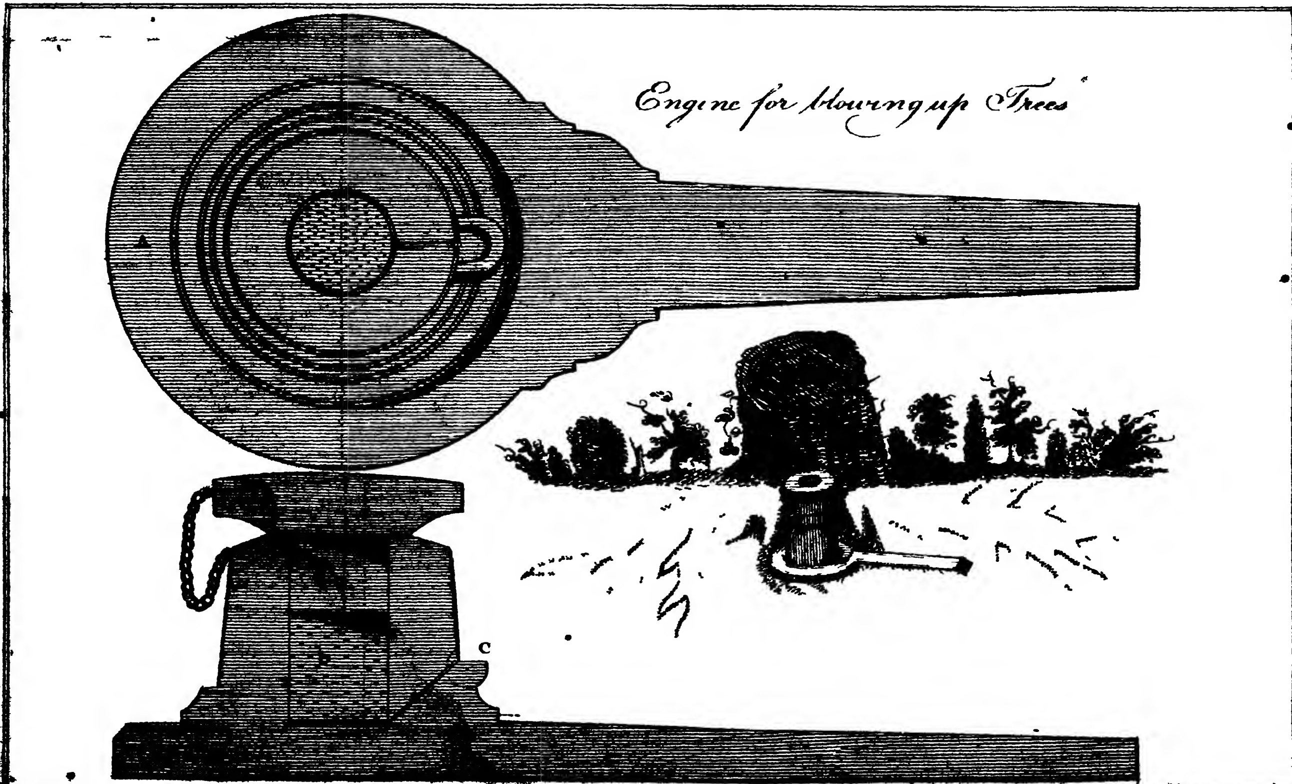
Mr E. Walker's reflecting Quadrant.



Mr Huddleston's method of conveying Boats &c.



Mutlow Sc. Russell Ct.



Mutlow Sc. Russell Court

Magazine Pistol, containing nine Charges.

Fig. 1.

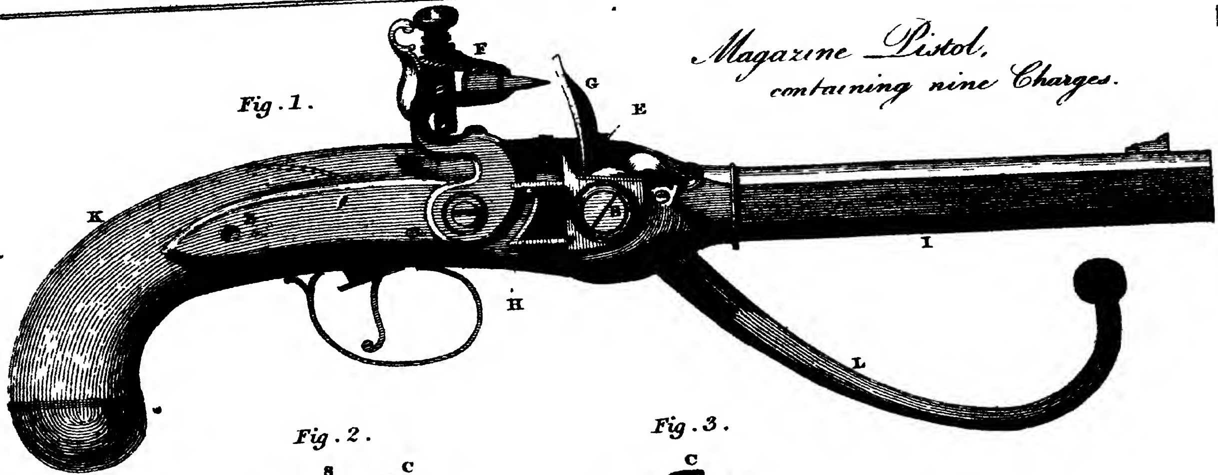


Fig. 2.

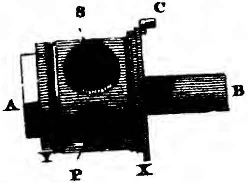


Fig. 3.

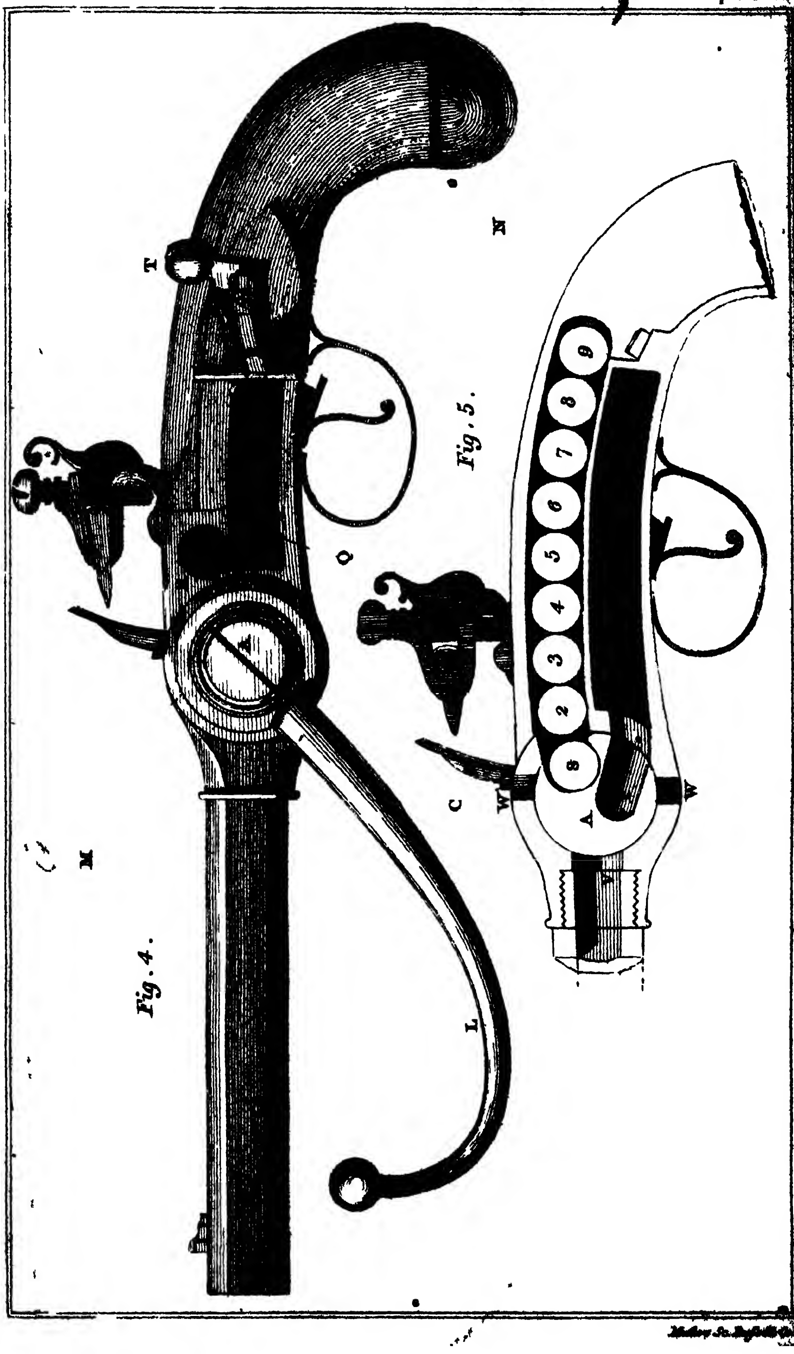


Fig. 4.

Fig. 5.

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END OF THE FOURTH VOLUME.

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